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THE

JOURNAL OF SCIENCE.

FEBRUARY, 1884.

I. PROFESSOR HUXLEY'S DARWINISM.

By OSWALD DAWSON.

"In England the leader of the Darwinian party was Prof. Huxley. . . . That the 'Origin of Species' was driven home to men's minds by an interpreter of such power, probably cut short by some years the interval of hesitation which necessarily preceded the period of full recognition."—PROF. L. C. MIALL.

"Darwin expounded and illustrated by Huxley is a most formidable combination, and one that might well quench the ardour and damp the courage of any one who wished merely to cavil."—DR. CHARLES ELAM.

IN 1860 Prof. Huxley, whom Dr. St. George Mivart tells us is "Mr. Darwin's authorised interpreter," assumed the "office of an interpreter between the 'Origin of Species' and the public," and he "acted for some time in the capacity of a sort of under-nurse" to the book. The appellation "under-nurse" is somewhat appropriate, for when celebrating "the coming of age of the 'Origin of Species'" by a lecture, Prof. Huxley did not mention the theory of Natural Selection,—a circumstance the more unfortunate inasmuch as *twenty years previously* he had remarked that "*twenty years hence* naturalists may be in a position to say whether" or no "species offer residual phenomena, here and there, not explicable by natural selection." According to the "Times" report of the Rede Lecture, 1883, Prof. Huxley's chief object on that occasion was "to draw a sharp line of demarcation between this great and well-vouched truth [Evolution] and all the innumerable collateral speculations which might stand or fall without touching the central core of the doctrine." This anxiety to divorce

collateral speculations from the major theory strikingly contrasts with Mr. Darwin's dexterity in confusing them.

The task here is an exposition from the technical standpoint of the several inversions and kindred tamperings with Darwinism committed from time to time by "Mr. Darwin's authorised interpreter."

The Parentage of the Origin of Species.

In 1859, in "Macmillan's Magazine," and in 1877, in the "Manual of the Anatomy of Invertebrated Animals," Darwinism seems to have meant natural selection; and "divergence" is spoken of as existing between "three views" held "among those who accept the doctrine of Evolution in its general outlines" ("Manual," p. 40). In 1880, when the "Origin of Species" came of age, "Evolution in its general outlines" apparently monopolised it, and, as remarked above and by others, natural selection was not mentioned. In 1878, in the transitional stage, the name of Mr. Wallace was thrice associated with that of Mr. Darwin, and Evolution was represented as the "irrepressible" successor of sundry conceptions chronologically intermediate between Treviranus and Lamarck and Darwin and Wallace ("Science and Culture," pp. 297, 306-308). In 1880 "the 'Origin of Species' [became] the logical sequence of the sequence of the 'Principles of Geology.' The fundamental doctrine of the 'Origin of Species'" ceased to be natural selection, or the preservation of favoured races in the struggle for life. Not a vestige of proof is offered to substantiate the implication that Mr. Darwin was ever one whit guided by deduction from the speculations of physical geology.

The Absolute and Pure Darwinian.

This "creature" ("Critiques and Addresses," p. 298) differs from the Darwinian Darwinian in that he ignores the effects of use, variation independent of natural selection, and so on. When therefore Prof. Huxley doubts whether he "can ever have seen one alive," one legitimately expects that Dr. Mivart is about to be reprimanded for "passing over the effects of the increased use and disuse of parts," &c. ("Origin of Species," p. 176). In truth, however, Prof. Huxley never mentions these topics, being himself the nearest approach to the censured creature perhaps ever yet seen alive. Let us quote him:—"What if species should offer residual phenomena, here and there, not explicable by

natural selection?" ("Lay Sermons," p. 298). "How far 'natural selection' suffices for the production of species remains to be seen. Few can doubt that, if not the whole cause, it is a very important factor in that operation" ("Science and Culture," p. 306). "But postulating the existence of living matter endowed with that power of hereditary transmission, and with that tendency to vary which is found in all such matter, Mr. Darwin has shown good reasons for believing that the interaction between living matter and surrounding conditions, which results in the survival of the fittest, is sufficient to account for the gradual evolution of plants and animals from their simplest to their most complicated forms, and for the known phenomena of Morphology, Physiology, and Distribution" ("Manual of the Anatomy of Invertebrated Animals," pp. 39, 40, and "Encycl. Brit.," art. 'Biology').

The Rôle of Natural Selection.

Prof. Huxley "never had the slightest notion" that it was other than "an iteration of the fundamental principle of Darwinism" to assert that the "action of 'natural selection'" is "subordinate" ("Critiques and Addresses," p. 299): our author maintains that variation is of paramount importance, although Mr. Darwin had applied, and repeated, an illustration in his "Variation under Domestication," to enforce the opposite lesson (1st ed., ii., 248, 249, 430; 2nd ed., ii., 236, 426).

The Causes of Variation.

Prof. Huxley tells us that Mr. Darwin "seeks for the principal, if not the only, cause of variation in the influence of changing conditions" ("Manual of the Anatomy of Invertebrated Animals," p. 40, and "Encyclopædia Britannica," art. 'Biology'), whereas Mr. Darwin tells us, in the "Origin of Species," that "Such considerations as these incline me to lay less weight on the direct action of the surrounding conditions, than on a tendency to vary, due to causes of which we are quite ignorant" (p. 107); in the "Descent of Man," that "in general we can only say that the cause of each slight variation and of each monstrosity lies much more in the constitution of the organism than in the nature of the surrounding conditions" (p. 608); in the "Variation under Domestication," that "these facts are important from showing, as remarked in a former chapter, that each trifling

variation is governed by law, and is determined in a much higher degree by the nature of the organisation, than by the nature of the conditions to which the varying being has been exposed" (ii., 344).

Truly there is *some slight excuse* for Prof. Huxley's misinterpretation, for Mr. Darwin does once affirm that "every variation is either directly or indirectly caused by some change in the surrounding conditions" ("Variation under Domestication," ii., 415), but insists so often that variations are very remotely related to changing conditions that he must have experienced some surprise when his authorised interpreter thus distorted his views. What can be more explicit than the following? "We are driven to conclude that such peculiarities are not directly due to the action of surrounding conditions, but to unknown laws acting upon the organisation or constitution of the individual,—that their production stands in hardly closer relation to the conditions of life than does life itself" (*Id.*, i., 448, 449), "life itself" being the Creator's breath.

Mr. Darwin's real teaching is as follows: changed conditions, acting often not indeed alone upon the individual, fœtus, or otherwise, but upon its "ancestors," render the organisation plastic, variations being the result of a certain "tendency to vary" thus induced; the nature of such variations being likewise generally determined by the constitution of the subjects of the influence rather than by the peculiar nature of such influence. This Prof. Huxley abbreviates into the statement that "Mr. Darwin . . . seeks for the principal, if not the only, cause of variation in the influence of changing conditions."

To remark that "it is quite conceivable that every species tends to produce varieties of a limited number and kind, and that the effect of natural selection is to favour the development of some of these, while it opposes the development of others, along their predetermined lines of modification" ("Science and Culture," p. 307), is either to say that it is quite conceivable that Mr. Darwin's view may be correct, or that Teleology is conceivable. The truth that "the importance of natural selection will not be impaired, even if" Darwinism be proved true (*Id.*, p. 307), must puzzle fellow-interpreters; while the alternative conception of a Master of the Mint manufacturing, "along their predetermined lines," coins which he knew would be obsolete when ready for currency, is as paradoxical as the converse proposition, namely, that a variety may be "perpetuated or even intensified when selective conditions are indifferent, or perhaps

unfavourable, to its existence" ("Critiques and Addresses," p. 299). Yet Prof. Huxley, without specially alluding to secondary sexual characters, tells us "now" (1871) Mr. Darwin's "inclination" is to admit that such *perpetuation* of variations, under circumstances *unfavourable to their existence*, may occur "in the teeth of all the opposition 'natural selection' can offer," as Dr. Mivart expresses it.

Teratology and Darwinism.

In 1860 Prof. Huxley wrote as follows:—"As a general rule, the extent to which an offspring differs from its parent is slight enough; but occasionally the amount of difference is much more strongly marked, and then the divergent offspring receives the name of a Variety." Hexadactylism and Ancon sheep serve to illustrate this. In the first generation of the *Gratio Kelleia* case were two pentadactyles, one hexadactyle, and one intermediate; in the second generation were six hexadactyles, one intermediate, and five *plus* "many" pentadactyles; once there occurred three pentadactyles and one hexadactyle, and on this Prof. Huxley observes that "the prepotency of the variety was still more markedly exemplified" than when three hexadactyle and one pentadactyle were born in a family.

As to Ancon sheep, Mr. S. E. B. Bouverie-Pusey writes as follows:—"Of the other, the ancon, or otter breed, said to have originated in Massachusetts in 1791, we know much less, as it is now extinct, and we have apparently only one account of it, from a Colonel Humphreys, written in 1819, which account we have no means of controlling or verifying. I suspect that there is some mistake or exaggeration in the alleged suddenness of the appearance of this breed, and that more time and more selection was really employed than appears. We know nothing of the breed of the sheep among whom the first ancon lamb was born, but it seems (*vide* Youatt) that the sheep of America were in a highly mongrelised state,—a condition* extremely favourable to reversion. We have also reason to believe that this form has been observed in Europe, and therefore may be considered as a race of unknown origin (*vide* Isidore Geoffroy St. Hilaire 'Histoire Naturelle')" ("Permanence and Evolution," pp. 51, 52).

Referring to the *Kelleia* family, Prof. Huxley remarks that

* According to Darwin "Animals and Plants under Domestication," vol ii., p. 13, *et seq.*

"If a variation which approaches the nature of a monstrosity can strive thus to reproduce itself, it is not wonderful that less aberrant modifications should tend to be preserved even more strongly" ("Lay Sermons," p. 267),—a paraphrase of Mr. Darwin's statement that "If strange and rare deviations of structure are really inherited, less strange and commoner deviations may be freely admitted to be inheritable" ("Origin of Species," p. 10), although pangenesis removes a rudimentary organ when much reduced ("Variation under Domestication," ii., 309, 393).

The Ancon sheep chosen by Prof. Huxley as a typical instance of "less aberrant modifications," Mr. Darwin has taken the opportunity to remark, "throw very little light on our subject" ("Origin of Species," p. 201); they are "semi-monstrous" ("Variation under Domestication," i., 104); in common with hexadactyle men their behaviour in respect to inheritance differs from that exhibited by races formed by selection and by hybrids (ii., 70, 72). We have seen that they lack an unassailable history; also that a mistake occurs in the recordal of the Kelleia case. Consequently those whose conversion to Darwinism was accelerated by Prof. Huxley's unintentionally unsuitable interpretation may fitly re-examine their views.

The Sterility Desideratum.

We will now proceed to examine a chimera with which Prof. Huxley encumbered Darwinism. Persons may be reluctant to believe this unless I discuss the topic at some length, especially, perhaps, since Prof. Huxley once reproached Dr. Mivart for "creating a difficulty for Mr. Darwin out of a supposed close similarity between the eyes of fishes and cephalopods, which (as Gegenbaur and others have clearly shown) does not exist" ("Critiques and Addresses," p. 253),—not to dwell on the fact that Dr. Mivart did not "create" the difficulty, but borrowed it with acknowledgment on more than one occasion, including the one quoted by Prof. Huxley, in both editions of the "Genesis of Species," from "Habit and Intelligence."

Unfortunately Prof. Huxley is not quite clear what the "little rift within the lute" really is. In 1860 "the production of mutually more or less infertile breeds from a common stock" was the desideratum; in 1863, in some lectures to working men which the author had "no leisure to revise," "complete physiological divergence" was demanded, and if it should be proven impossible to obtain this

"Mr. Darwin's hypothesis would be utterly shattered." In "Man's Place in Nature" (pp. 107, 108) the little rift was still unhealed. In 1864, though still "the weight of this objection is obvious" ("Lay Sermons," p. 308); "the difficulties presented by hybridism" were classed among "details," "so battered and hackneyed on this side of the Channel that not even a Quarterly Reviewer could be induced to pick them up for the purpose of pelting Mr. Darwin over again" (p. 318); though it is not obvious why the alleged terror of Quarterly Reviewers should have induced an authority such as M. P. Flourens, when conducting an "Examination du Livre de M. Darwin sur l'Origine des Espèces," to ignore Mr. Darwin's ninth chapter; nor did such alleged terror of Quarterly Reviewers deter Prof. Huxley from reprinting from the "American Cyclopædia" his statement that "what is needed for the completion of the theory of the origin of species is, first, definite proof that selective breeding is competent to convert permanent races into physiologically distinct species," in the same issue of the "Popular Science Monthly" in which appeared the editorial announcement of certain lectures in which some fossil horses were pronounced demonstrative evidence of evolution (August, 1876). It had become expedient to offer a modified estimate of the necessity of manufacturing mutually infertile breeds from a common stock; else how good a cartoon for a scientific "Punch"—the triumphant horse and the obstreperous mule trotting out to tournament, the one as "demonstrative" evidence of evolution, the other concerned in some plot to "utterly shatter" that doctrine.

It will be observed that the "utterly shattered" quotation is but an intensified repetition of what may be an erroneous view taught in the "Lay Sermons." Were it not so I should be more reluctant to quote from an unrevised volume, though it is one of the four "chief works consulted" by Miss Arabella B. Buckley in the composition of successive editions of her "Short History of Natural Science." In this work "young and unscientific people" are informed that the "one stumbling-block" to Darwinism is the circumstance that "we have never yet been able to trace out two varieties of an animal which have become so different that they do not pair together;" and as the author proceeds to discourse upon choice, and never mentions sterility, one assumes the desideratum is distinct from that sought by Prof. Huxley,—not the production of physiological, but of emotional, properties;

though those able to appreciate the difference between the "only one difficulty" and "almost the only *real* one" may be more competent to determine whether Miss Buckley's desideratum is a counterfeit of Prof. Huxley's or no.

The inaccessible nature of the evidence destined, if forthcoming, to utterly shatter Darwinism, is its conspicuous feature; something must be proven impossible; though whether a dozen, a score, or a hundred failures will suffice to prove the impossibility of breeding races mutually infertile from a common stock, no notion whatever is afforded. The fabric resembles the logic of the desideratum. First comes the assumption (not explicitly adopted by Prof. Huxley in its integrity) that "species" under Nature are sterile, much evidence to demonstrate the unwarrantableness of which is given by Prof. Karl Semper, in his contribution to the "International Scientific Series"; and then the assumption that "the antipathy of wild animals of different species for one another, or even of wild and tame members of the same species, is ordinarily so great that it is *hopeless to look for* such unions in Nature" ("Lay Sermons," p. 273). Evolutionists possess, however, some very hopeful evidence on this point; Prof. Semper cites above half-a-dozen such instances, and Mr. Darwin tells us that "a female Australian dingo in England attracted the wild male foxes" ("Variation under Domestication," ii., 80), also that "the Indians of North America cross their half-wild dogs with wolves" (i., 21); again, "strange as the fact may appear, many animals under confinement unite with distinct species as freely as, or even more freely than, with their own species" (ii., 133, &c.), and he gives evidence that certain animals breed more successfully with other species than with members of their own. These several facts are not calculated to sustain the expectations of those who "will go so far as to express our belief that experiments, conducted by a skilful physiologist, would very probably obtain the desired production of mutually more or less infertile breeds from a common stock, in a comparatively few years," when "it took Mr. Wicking thirteen years to put a clean white head on an almond tumbler's body, 'a triumph,' says another fancier, 'of which he may be justly proud'" (*Id.*, ii., 183). Prof. Huxley's "comparatively few years" ("Lay Sermons," p. 295) means "a long time" (p. 273).

In the circumstance of the difficulty of crossing the common rabbit and *Lepus Huxleyi*, Prof. Haeckel discerns the realisation of Prof. Huxley's wish. Mr. S. E. B. Bouverie-Pusey, however, offers criticisms, and adduces evidence that

the Porto Santo rabbit did not "as certainly originate since the year 1420," as Mr. Darwin believes.

But the strongest argument (to many readers) against the validity of Prof. Huxley's objection is yet to be mentioned, namely, that Mr. Darwin himself, so far from believing that his theory would be utterly shattered by the everlasting failure to obtain the desideratum, insisted on more than one occasion—even in the very first edition of the "Origin of Species"—that "as domestication (I do not mean mere confinement) apparently tends to eliminate sterility, we ought not to expect it also to produce sterility" (p. 461): again, he writes—"Some eminent naturalists believe that a long course of domestication tends to eliminate sterility in the successive generations of hybrids which are at first only slightly sterile; and if this be so, we surely ought not to expect to find sterility both appearing and disappearing under the same conditions of life" (p. 269; in 6th ed., pp. 257, 263, and 405). These quotations are utterly ignored by Prof. Huxley, who actually declares that "Mr. Darwin is perfectly aware of this weak point" ("Lay Sermons." p. 295).*

"Certain naturalists," writes Mr. Darwin, "have recently laid too great stress, as it appears to me, on the difference between varieties and species when crossed," so he forthwith proceeds to levy an ingenious argument against these anonymous naturalists ("Variation under Domestication," ii., 174, 175). He enquires, "Why, then, should these [differences in fertility] be thought of such paramount importance in comparison with other incidental and functional differences? No sufficient answer to this question can be given." Mark, *en passant*, Mr. Darwin's dogmatic style.

Now the circumstance that "there is no certain evidence of offspring ever having been produced by a male and female mule" ("Lay Sermons," p. 272) seems to be the corner-stone of the desideratum. It may not be the happiest illustration, for the horse and ass *belong to different genera*. Possibly the progeny of horses and rhinoceroses, or horses and hippopotami, are sterile.

But supposing a skilful physiologist rendered an imaginary extant common ancestor of the horse and ass the bifurcating

* In the "Variation under Domestication," Mr. Darwin remarks that the fertility of the varieties of domestic animals, when crossed *inter se*, "is extraordinary," but "not so extraordinary as it at first appears" (ii., 82). *Cf.* "Origin of Species," p. 256, to same effect; also p. 236. Miss Arabella B. Buckley in her counterfeit presentation of the desideratum adopts a similar procedure.

stem of brand new horses and asses, whose progeny were sterile *inter se*; yet supposing both these novelties neighed, or both brayed, another series of experiments would be necessary to prove that diversity of voice is not diagnostic of special creations alone. Further, since "it *matters not one whit* whether this sterility is universal or whether it exists *only in a single case*" ("Lectures," p. 146), human language is as indictable as mule sterility; yet Prof. Huxley would assuredly be reluctant to join the school of folks who demand that the orang-outang shall converse with his keeper before the doctrine of our Simian origin shall be conceded qualified for other than provisional acceptance!

It may be incidentally appended that the mule exhibits very much that dilatoriness of reproductive power, so to speak, which an evolutionist might anticipate. Female mules may cross with the parent horse. In France, some years ago, a female mule produced foals two years in succession to an Arab sire, "Land and Water" informs me; while the view that male mules are impotent is partially contradicted by facts, for the hybrid from an ass and zebra has crossed with a mare ("Variation under Domestication," ii., 16). Nor can the testimony of Aristotle be summarily rejected, since he not merely affirms their fertility with mares, but specifies the age known as puberty in our own species, and in addition gives the proper name for the progeny ("History of Animals," Book VI.).

As Prof. Owen, annotating John Hunter (the dog-wolf progeny of a friend of whom re-crossed with a dog), has remarked, this capacity of a hybrid to cross with a pure species has not the same import as fertility *inter se*. The mule likewise corroborates the Pallasian doctrine, an extension of which, as we have seen, interferes with the "little rift within the lute"; for "Dureau de la Malle, who has so closely studied classical literature, states that in the time of the Romans the common mule was produced with more difficulty than at the present day" ("Variation under Domestication," ii., 88).

Finally, if mules were fertile, *inter se*, the advocate of the creation of physiological species could retreat a step or so, and insist upon the production of jumarts fertile *inter se*,—a jumart being the reputed offspring of a bull and a mare. But I am trespassing beyond the limits of this section—the discussion of *the sterility desideratum*.

The American Addresses and Darwinism.

The previous section shows that Mr. Darwin recognised in the phenomena of hybridism nothing subversive to *Evolution*, though he conceded the sterility of hybrids is little, if at all, explicable by *Natural Selection*. Now the theory which was to be utterly shattered, if the logical feat of proving something impossible were accomplished, was, according to Prof. Huxley, "Mr. Darwin's theory," and this association of the sterility desideratum with Mr. Darwin's theory has justified its examination here. The horse of the "American Addresses," on the other hand, is demonstrative evidence of "Evolution;" so it may suffice to remark that a portion of this same pedigree is utilised by Dr. Mivart to substantiate his saltatory hypothesis; to hint how suitably it may be chosen to illustrate the effects of "use"; to point out that if parallelism of specific evolution, independent of inherited tendency to vary along parallel lines, be animpediment to Darwinism, horses possibly furnish an impediment; also to remind the reader that if, as urged by Dr. Charles Elam, it is permissible for a specific creationist to maintain the specific identity of *Equus* and *Orohippus* (*Eohippus* too?), the propriety of reversing matters, and pronouncing fossil *Sauropsida* "demonstrative" and fossil horses "favourable" evidence of evolution, suggests itself;* and to remark that if the occurrence of special creation and special extinction be not improbable, *à priori*, the latter may be summoned to explain the absence of aboriginal American horses.

But the "American Addresses" are by no means devoid of Darwinism, inasmuch as its supremacy is maintained with no less confidence than in "Man's Place in Nature," the reason assigned for this supremacy being that persistence is compatible with it; and as any evolutionary theory in which progress is made a necessary contingent suffers patent suicide, Darwinism seems naturally preferable. The truth is, however, that the "neutrality" of the testimony of persistent types is debateable, and that it would have harmonised better with Mr. Darwin's own views had none persisted, as the following quotations from the "Origin of Species" will show:—

* *Eohippus* was described by Prof. Marsh the month following the delivery of the American Addresses; and not only it, but *Orohippus* and *Mesohippus* may be dispensed with without interfering with the "demonstrative" nature of the evidence (p. 84). *Vide* also "Manual of the Anatomy of Vertebrated Animals" (p. 305).

"Such a case would imply that one form had remained for a very long period unaltered, whilst its descendants had undergone a vast amount of change; and the principle of competition between organism and organism, between child and parent, will render this a very rare event" (pp. 265, 266); "they will, during each successive age, have to be slightly modified so as to hold their places in relation to slight changes in the conditions" (p. 308). "When any of the inhabitants of any area have become modified and improved, we can understand on the principle of competition, and from the all-important relation of organism to organism in the struggle for life, that any which did not become modified and improved would be liable to extermination" (pp. 291, 292). And that Prof. Huxley, as a Darwinian, would have welcomed with more composure the absence of such persistence, may be inferred from the fact that he speaks of the existence of these types as a "*circumstance to be wondered at*" ("Lay Sermons," p. 219). This was in 1862, when "observation and experiment upon the existing forms of life" was "the only way in which it [progressive modification] can be demonstrated" (p. 226). In the following year, when lecturing to working men, Prof. Huxley pronounced the circumstance "something stupendous."

Supposing persistence explicable by Darwinism, how is the supremacy of that theory thereby established? That earlier speculators may have considered progress a necessary contingent no more damages the pith of their view of modification than the circumstance that Mr. Darwin restricted the mode of action of natural selection will cause the general theory of selection to be annihilated should the restrictions imposed by him prove to have been unwarranted,—a question beyond the scope of this article.

It is singular that once upon a time Prof. Huxley, if I understand him aright, insisted that—viewed from another standpoint—persistent types furnish *not neutral*, but, by discrediting catastrophic and periodic-creation theories, *very favourable* evidence of evolution. Had all Prof. Huxley's evolutionary writings been inspired by the moral wisdom of the brief paper of June, 1859,* he would never have earned

* It is possible that Prof. Huxley was here advocating the view that the entire modern fauna and flora have persisted from a remote geological epoch, but that, owing to "the imperfection of the geological record," only a few forms are known to have done so. The important passages are given in the "Historical Sketch" of the "Origin of Species"; in addition to which is the concluding sentence, which runs as follows:—"In fact, palæontology and physical geology are in perfect harmony, and coincide in indicating that all we know of the conditions in our world, during geological time, is but the last

the ingratitude of evolutionists by the installation of palæontology as "the only perfectly safe foundation for the doctrine of Evolution" ("Manual of the Anatomy of Invertebrated Animals," Introduction, p. 41). Those who prefer to advocate or refute the doctrine of our Simian origin not as a mere deduction, or upon the strength of the sparse palæontological material at present available, have especially reason to be vexed.

Preceding the epilogue is a digression on Geological Time, wherein "the uninformed" are reproached for discerning "a solid foundation" for the belief that sufficient past time is not at the evolutionist's disposal.

"I desire to be informed," said the lecturer, "what is the foundation for the statement that evolution does require so great a time"; but Mr. Darwin never complied with the request. His uninformed readers may, however, answer as follows:—"We borrowed from the 'Origin of Species' itself our information that '140 million years *can hardly be considered as sufficient*' for pre-Cambrian evolution, and that 60 million years 'appears *a very short time* for' post-Cambrian evolution (p. 286). Further, we find it repeatedly insisted upon in the 'Origin of Species' that evolution by natural selection must be an extremely slow process, while on pages 269, 270 we notice a passage which we think it legitimate to interpret as meaning that under Nature we must not expect to find a breed '*sensibly* changed in the course of *two or three centuries*.'"

One smiles when watching Prof. Huxley thus correcting "the uninformed," Mr. Darwin combatting "certain naturalists," and neither mentioning the other personally.

The Simplicity of Darwinism and Teleology.

That Prof. Huxley should have found out that "the 'Origin of Species' is by no means an easy book to read—if by reading is implied the full comprehension of an author's

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term of a vast, and, so far as our present knowledge goes, unrecorded progression" ("Proc. Roy. Inst.," June 3rd, 1859). *Cf.* that speculative treat "Illogical Geology" in Mr. Herbert Spencer's "Essays: Scientific, Political, and Speculative" (vol. i.), published the following month in the "Universal Review." Or—and I had thus understood Prof. Huxley when writing this section—it may be that he considered persistent types favourable evidence, inasmuch as he thought philosophers would be reluctant to ascribe to the Creator the performance of an operation so whimsical as the repeated destruction and re-creation of a few favoured forms,—or their specially permitted survival of catastrophies extirpating the remaining fauna.

meaning" ("Lay Sermons," pp. 256, 257)—is not surprising: he calls it "a mass of facts crushed and pounded into shape, rather than held together by the ordinary medium of an obvious logical bond"; the bond being "often hard to find," and only discoverable on the application of "due attention." When understood, however, "the Darwinian hypothesis has the merit of being eminently simple and comprehensible in principle" (p. 292); furthermore, the general view of the universe is simpler than that offered by the advocates of specific creation (Lecture VI. of the "Lectures to Working Men"). Mr. Darwin, on the other hand, does "not wish to lay much stress on the greater simplicity of the view of a few forms or of only one form having been originally created, instead of innumerable miraculous creations having been necessary at innumerable periods" ("Variation under Domestication," i. 12).

This question we cannot pursue, nor can we here discuss any niceties of distinction between Teleology as commonly understood and Teleology as otherwise understood, but simply point out that "that which struck the present writer most forcibly on his first perusal of the 'Origin of Species' was the conviction that Teleology, as commonly understood, had received its death-blow at Mr. Darwin's hands" ("Lay Sermons," p. 301), whereas Mr. Darwin maintains that "whether or not" slight variations, &c., are ordained, the outcome manifests purpose ("Descent of Man," p. 613). 3+5=8 (an even number). But unfortunately teleological intuitions are less persuasive than arithmetical calculations, even when fortified by Bridgewater Treatises, Analogies of Revealed Religion, correspondence with celebrated divines, and proficience in divinity ("Origin of Species," ii., 422) for a pamphlet,* and sundry periodicals,—*e.g.*, the "Academy,"—have made public the fact that Charles Darwin gave up Christianity when 40 years of age, and that he was with avowed Atheists in thought, though he preferred the term "agnostic." Visitors to Westminster Abbey may calculate that his fortieth birthday anniversary was a decade anterior to 1859.

Mr. Darwin's Reticence.

Prof. Huxley has judiciously abstained from allowing to his review of Prof. Haeckel's "Evolution of Man" ("Academy," January 2, 1875) the publicity accorded to his critique on the "History of Creation," namely, incorpora-

* The Religious Views of Charles Darwin. By EDWARD B. AVELING, D.Sc. Freethought Publishing Company. 1883.

tion in a collection of essays. The notoriety of the review reposes on the assertion that concerning our Simian origin "Mr. Darwin has had leisure to state his views more fully, though not more distinctly than in the 'Origin of Species,' in the 'Descent of Man,'" and the repudiation with unqualified scorn of the allegation that Mr. Darwin had been reticent on the topic.

"Much light will be thrown on the origin of man and his history." Is this *the most distinct* mode in which Mr. Darwin proclaimed our Simian origin? Was it so monstrous a supposition that the philosopher who required a Creator to breathe life into primeval forms might not also stand in need of a little supernatural aid to make a man? Not reticent when, in the first edition of the "Origin of Species," it was announced that sexual selection had probably contributed to *differentiate* mankind, and when not a hint was given that it may also have assisted in *originating* our species? Not reticent when "during many years I collected notes on the origin or descent of man, without any intention of publishing on the subject, but rather with the determination not to publish"? Not reticent when,—that his coward kin might inhale with lessened shock the breath which vivified the primordial one, or few, whose progeny were to be for countless æons of a yet-continuing Time, "from the hour of their birth to that of their death," subjects of a struggle involving scenes which to the "imagination" of him whom Prof. Huxley declares "left untouched" the question of the origin of life, it were "FAR MORE SATISFACTORY* to look at . . . not as specially" ordained, and evolving "contrivances . . . abhorrent to our ideas of fitness,"—not reticent, I ask, when, to compromise with his victims, he first announced that natural selection, besides forming species, might affect "*even* orders," subsequently that *classes* might be involved, and that "at the most remote geological period the earth may have been as well peopled with many species of many genera, families, orders, and classes, as at the present day," till by-and-bye he conducted them to the "four or five" patriarchal animals (i., radiate patriarch; ii., molluscan ditto; iii., articulate ditto; iv., vertebrate ditto; v., Adam—why not?) and the "equal or less number of plants, when at length the "probably deceitful guide" Analogy broke the barrier between the two kingdoms, and apparently also between the living and the non-living, disclosing the "laws impressed on *matter* by the Creator" and

* This quotation is copied here from the "Origin of Species."

the "few beings which *lived*" before the deposition of the Silurian; this penultimate scene finally yielding to the restitution of the renowned Creator's breath ("Origin of Species," 1st ed., pp. 125, 191, 126, and the 'conclusion').

Will one who dare deny Mr. Darwin's reticence be dismayed by an anachronism? Not so. In "Critiques and Addresses" (xii.) Mr. Wallace is made to pose as one of "Mr. Darwin's critics," notwithstanding the circumstance that his "contributions" were incorporated in book-form in 1870, the year before Mr. Darwin's "Descent of Man" was published. He may have been a "critic" of the "Origin of Species"!

Concerning natural selection and Dr. Mivart's opinion that that theory is associated more exclusively with Mr. Darwin's name on account of Mr. Wallace's reticence, Prof. Huxley, after pronouncing the opinion "simply ridiculous," remarks that "if there was any reticence at all in the matter, it was Mr. Darwin's reticence during the long" time he postponed publishing the "Origin of Species"—that is, till he availed himself of the circumstance that Mr. Wallace communicated his discovery to the Linnean Society through Mr. Darwin himself.

Whether Mr. Darwin was prolonging his reticence for the purpose of accumulating factual evidence, or whether he deemed "the masses not yet ripe" for the promulgation of his theory, Prof. Huxley does not speculate; nor need we here, for the expediency of reticence or aggressive candour, the propriety of pious frauds and their Darwinian analogues, concern us here as little as the "Nature" Memorial Notice.

II. THE DEVELOPMENT OF OXYGEN BY PLANTS AS AFFECTED BY THE INTENSITY OF LIGHT.

IT is popularly known that light, and of considerable intensity, is needed for that most important process of plant-life the decomposition of carbonic acid and the liberation of pure oxygen. That such an operation takes place few will care to dispute. That its results approximately compensate for the respiration of animals, though not proved, is generally admitted. But the series of experiments hitherto conducted with a view to ascertain the laws of this phenomenon left much to be desired. Accordingly the eminent botanist J. Reinke has made a further investigation of the subject, the results of which we will here summarise.

As a measure of liberation of oxygen he took the number of air-bubbles thrown off by a water-plant in a given time. He threw upon the plant light concentrated by means of a lens, measuring the intensity in each experiment by the distance of the plant operated upon from the focus of the lens. As the subjects of the experiments he selected the points of young shoots of the well-known water-weed *Elodea Canadensis*, which were found to throw off a constant current of air-bubbles equal in size. These shoots were placed in well-water through which a current of air had been passed for a short time. The rays of light were caused to fall normally to the surfaces of the leaves.

Observations were made both with increasing and decreasing intensities of light. The plant was exposed to each degree of intensity for half to one minute before the result was taken, and the number of bubbles was then counted in two intervals of a quarter minute each. The temperature did not rise during the experiments more than 3° C., and ranged during the different sets of observations between 20° and 21° C.

None of the plants examined gave off gas-bubbles at intensities of light equal only to 1-24th of normal sunshine. Between the intensities of 1-16th and 1-4th the number of bubbles increased proportionally to the light,—*i. e.*, from four to twenty. A corresponding, but of course inverse, phenomenon was observed in the decreasing series. But at the intensity 1 (= normal sunlight) there was only twice as

much gas given off as at ½, and on further increasing the intensity to 8 the quantity of gas eliminated remained constant.

There is therefore a maximum speed of liberation which is reached probably below the intensity 1, and on further increase of intensity neither rises nor falls. This maximum may also be called the optimum point, but the light-optimum differs from the temperature-optimum by remaining constant on further increase of light.

Herr Reinke gives the results of his experiments on different plants between the intensities 1-16th and 16, in seven tables, and sums up the results in the following proposition :—"The liberation of gas from *Elodea*, depending upon light, begins at a medium grade of illumination, and rises with the increasing intensity of light to a maximum which corresponds approximately with direct sunlight, and is reached sometimes at a slightly lower, and at other times at a slightly higher, intensity, whilst a further increase of intensity produces no further increase of the liberation of gas."

Herr Reinke next emamined the liberation of oxygen on a further augmentation of the intensity of light up to the point at which the destruction of the chlorophyll and the death of the cells occur. For this purpose a large lens was employed, and the distances were calculated for the intensities 1 (= normal sunlight) up to 64. In order to guard against the rise of temperature due to the concentration of the sun's rays, which would complicate the results, the light was passed through a trough 9 inches in width, made of plate glass, and filled with a saturated solution of alum. During the experiments the rise of temperature did not exceed 4° to 6° C.

These experiments showed that up to very great intensities the number of the gas-bubbles given off was constant. Nor could any difference in the rate of the liberation of bubbles be detected when ordinary sunlight and such light concentrated sixty-four times were allowed to fall alternately upon the plant. In the most concentrated focal light a spray of *Elodea* continued to give off thirty-eight to forty gas-bubbles for a quarter of a minute, as before. Not until after an exposure of two minutes did the current begin to slacken. Fewer bubbles escaped, and at irregular intervals. After five minutes the development of gas came to an end, and on microscopic examination it was found that a great part of the surface of the leaflets was entirely bleached.

From these experiments it is plain that, even in the most

concentrated light of the focus of a burning-glass, the evolution of gas goes on at its ordinary maximum rate until the injurious action of the light becomes manifest and the chlorophyll is destroyed.

As regards the theory of the action of light, Herr Reinke deduces from his researches the following conclusions:— He believes that the liberation of gases observed in his experiments is an approximately accurate standard for the rate of the elimination of oxygen, and may thus again serve as an approximate expression for the decomposition of carbonic acid. He considers that the consumption of oxygen in the green cell vanishes in comparison with the liberation of oxygen. This fails to hold good at low intensities of light only when the proportion between the oxygen produced and consumed is different.

The fact that in intense and increasing light the liberation of oxygen remains constant in quantity, remains to be examined in the light of the various theories concerning the *modus operandi* of chlorophyll.

According to one view the chlorophyll acts physically as a protective screen during the decomposition of carbonic acid. It is here assumed that an increased oxidising action of the oxygen proceeds parallel with the growing intensity of the light. As now the above experiments prove that even in the most intense light there is neither an increase nor a decrease of the liberation of gas, we must suppose either that (1), with the increasing intensity of light, the decomposition of carbonic acid and the oxidation increase exactly proportionally, which, considering the complete mutual independence of the two processes, is little probable, especially as such a connection certainly does not appear during a decrease of the intensity of light; or (2), we must assume that the curve of the intensity of oxidation is quite different from that of reduction, so that in the intense light the former diverges much from the latter, and that therefore the gas which is given off in the most intense light contains an increased proportion of carbonic acid.

Herr Reinke has submitted this consequence of the latter assumption to an experimental test. He collected the gas which was given off in 1½ hours from *Elodea* sprays exposed to an intensity of light between 64 and 300 times that of ordinary sunlight. The temperature of the water had risen during the experiment from 20° to 29° C., and at the conclusion the two upper sprays, which had been most intensely illuminated, were completely bleached, whilst the others remained perfectly green. The collected gas amounted to

4·2 cubic centimetres, and contained 5 per cent of carbonic acid, or not more than is found during the action of ordinary sunlight: the remaining 95 per cent consisted of oxygen, with an undetermined admixture of nitrogen. Hence Reinke infers that the liberation of oxygen observed does not well agree with the theory of a physical mode of action of the chlorophyll.

If we start with the assumption that the activity of the chlorophyll in the decomposition of carbonic acid is chemical, this chemical function of a given mass of chlorophyll cannot—under the most favourable conditions—liberate more than a certain absolute quantity of oxygen. This exactly agrees with the result of the above experiments. It appears that the maximum liberation of oxygen from *Elodea* occurs at an intensity of light sometimes a little higher, sometimes a little lower, and sometimes coinciding with that of normal sunlight. A further increase of the intensity remained ineffective, but no injurious effects upon the liberation of oxygen were perceived until the chlorophyll was destroyed by the oxidising action.

It has been known with certainty since the end of the last century that the liberation of oxygen by plants is a function of chlorophyll. The direct proof has only lately been given by Engelmann that oxygen is given off exclusively by the chlorophyll granules, and that these alone act as organs of the decomposition of carbonic acid in the cell. A further step is thus taken rendering it probable that no substance other than chlorophyll under the impulsion of sunlight chemically mediates the decomposition of carbonic acid. We know that this process takes place in the chlorophyll granules only, and that consequently some substance present in these granules must be the effective agent. Hence it seems most natural to ascribe this function to the chlorophyll itself.

It is not sought to deny that, in addition to this function, chlorophyll may play an important part in protecting the cell from light. Along with the principle of the division of labour in Nature we encounter also the principle of the cumulation of functions. That such a function of protection from light may have profound significance for a plant is shown, *e.g.*, by the investigations of Berthold. Many sea-weeds escape the direct action of sunlight by their locality, whilst others equip themselves with the most manifold and curious protective arrangements. Wherein lies the injurious effect of direct irradiation, which is

doubtless the foundation of all these adaptations, it does not yet appear. That high light-intensities do not depress the decomposition of carbonic acid is certain; that ordinary sunlight effects no increased combustion appears also certain. Other effects must therefore exist by which intense light is injurious to many plants.

III. LATENT THOUGHT.

By D. Y. Cliff.

REMARKABLE incidences of parallelism in thoughts are constantly occurring in every-day life, and in the wider field of human history, where whole races "think alike," and often it is the civilised world. Is it not a wonder a man is never born who thinks totally different, not only in style, but in logical sequence, to his fellow-man? Nor is this nonsense, for I believe it is J. S. Mill who has said it is conceivable that there may be a species in the solar system with whom two and two make five.

It is worthy of notice, if we will think on the matter, how every thought we think is mentally expressed in our vernacular. Unpremeditated sentences spring out of our minds without an effort, but they must have been arranged internally before directing the vocal muscles. Is a thought spoken before it is considered, however swiftly?

I am inclined to think there is a continuous train of thoughts in our mind of which we are not aware, either of its existence or its laws,—a necessary result, so to speak, of the brain or nervous "molecular motion" or "vibration," and it is only when we require "to language" it that it is brought into activity. Surely mental activity must be constant, as we see with dreams: perhaps we are only aware of dreams in the interval between awaking and sleep.

I have heard of missionaries who became so used to the barbarous tongue in which they taught that they came to "think in it." Is there latent thought? This latent thought would naturally be hereditary in a family, a race, a nation, or even the human species, and would do away with

any astonishment that no logical "confused" man is born. The brain, being part of the body, cannot be exempt from the same causes that rule the body—evolution, correlation, &c. Of course this latent thought is a very dim, vague sort of a thing to define.

It would seem only certain that a constant relation, or proportion, is generally a law of thought. For instance, two persons look at a colour, and call it "red." How are we to prove that they both see identically the same effect? Each always sees one *constant result*, and he has learnt to call it "red," and so with the other person, although it may really be green to one and blue to the other, in their relative individual judgment. This of course is no detriment to our thinking, but it shows there may be wide differences unknown to us. Perhaps every one of us has a different picture of a man; we have only learned things relatively to ourselves. We pity the poor frog who, with a partially brainless skull, answers with a meaningless croak the stroke on his extremities; but are we much better—*i.e.*, any different—in principle? It is but a more complex affair. We get a kick, and our anger is roused, all our nervous system is disturbed, and the tongue utters harsh sounds, like the frog croaked. Then there is the restraintive power of the human mind; but animals have that too.

In this latent thought lies the nature that maketh all the world akin; how alike is every human emotion! Is there any difference in the love language of a Savage and an Englishman?

There are certainly traits of (latent?) thought in a nation's literature. How dull we find Oriental history, until it has time after time got thoroughly imbued with an European way of telling its story. Strange it is, too, what the effect of style is on writers; extracts from dry books seem to keep dry; writers on Oriental history (*e.g.*, China) seem unable to throw off its tone. We have literal translations of Chinese stories; what "stuff" they seem to the English mind! And yet often they approach very near. Colenso gives a Gala religious rhapsody; it is as good as a Psalm (which are by-the-bye the product of a very different stage of civilisation to the present).

We cannot help being struck by the art productions of the Aryan, Semitic, or Hamitic division of the human race. The difference is most striking. One would venture to suggest that the sculptural remains of ancient nations might be taken as decisive testimony as to their respective races.

The stiff, hard drawing of the old Egyptian, Assyrian, &c., can never be confounded with the more exact conformity to the natural shapes of the Europeans, and most decidedly stamps this relative "latent power" of the respective races. And here it is not entirely "surroundings" that have formed their ideas of beauty—a principle one of your correspondents argued for in a former number. This might explain the very peculiar drawings of the Chinese or Japanese, but cannot be applied to the ancient world, unless we unfoundedly suppose that their forms—contours of limbs—were as harsh as designed, which is absurd, and would not have escaped the notice of the early European historian. The hideous sculptures of Indû gods can hardly be the work of an Aryan race. Nor is it an accidental turn of thought like the nepotism of Travancore, Cochin, &c.

The human mind cannot conceive anything, as Mr. Cook, of Boston, has shown, and the "why?" of that question is only to be answered on the hereditary principle, our ancestors not being able even to guess the confines of space and time: yet still there are moments when we fancy we have an idea,—but the world is in our brain.

IV. ON SANITARY REFORM.

By Rev. SAMUEL BARBER.

III.

IN the case of children receiving elementary education the Government has demanded that a certain cubical space shall be assigned to each individual: this is a grand principle of sanitation now recognised and enforced by the Law. Inspection of lodging-houses is partly based on the same truth, *viz.*, that oxygen is essential to human life. Let us hope that this scientific axiom may receive a still further recognition at the hands of legislators, and that the inspection may become still more comprehensive. Why should school-buildings and lodging-houses monopolise the attention of sanitary authorities when ordinary dwelling-houses are shamefully overcrowded and vilely ventilated?

Why should a manufacturer suffer penalties for discharging noxious gases in a town when the local authority is suffered, with impunity, to ventilate senselessly-constructed sewers by opening, in front of our parlour-windows, apertures for the exit of a poisonous gas much more evil in its effects than the hydrosulphur fumes which chemists delight in. A case is before the writer at present, in a town (let us call it Blackhaven) where several of such wretched ventilators discharge their disgusting effluvia right in front of a terrace of houses, which become at times, in consequence, hardly habitable. To add to the indecency, a large school pours forth its crowd of children close to the place, and nothing delights them more than to carry on their games right over these pernicious and abortive devices.

It would appear to an ordinary mind that the *raison d'être* for a sewer is that it shall remove far away those dangerous results of organic life which, strictly speaking, it is the office of earth to neutralise; but if you discharge excreta into a drain, and then ventilate that drain by letting off the gas generated within it right among a crowd of human beings, you commit an absurd action, and are likely to receive, as a recompense, your labour for your pains. And here it may be right to enquire, in the name of mechanical science, if it is a thing impossible so to trap the outside pipes that a method of *driving* out the gas may be applied; and also whether a little more design cannot be exercised as to the position of the outlet pipes and the *elevation* at which they operate? We are accustomed to be told by engineers, concerning almost every variety of mechanical blundering in sanitary matters, that there is no want of power on the part of Science to apply a remedy; it is only a *financial* question. There is doubtless much truth in this remark. And we may again take note how much the conditions of human life are affected by the ideas of "what ought to be done" possessed by those in power; in other words, by the character of their moral and physical standards. This is a fact which may be realised in its physical bearing with regard to what we term "dirt," by an examination of the various aspects of scavenging presented in our different towns. Clearly, either the local regulations must vary vastly, or the ideals of purity possessed by local magnates must be almost as wide as the poles asunder. Thus we often hear that such a place is a "clean little town," another a "filthy hole." This differentiation in the matter of dirt may be witnessed by comparing the better class of towns in the South of England with those, for instance, which lie on the Cumberland coast.

There is one of these latter (which we will not name) where the prevailing notion with regard to "Matter in a wrong place" seems to be briefly comprehended in a saying once used to the writer by a cottager whom he was visiting:—"It's *all* clean dirt, sir." In this town, where'er the "casual eye" of the visitor is cast, solid and liquid filth of various dye arouses his slumbering indignation, and his final conclusion, after a full survey of the sanitary conditions of the place, is that "these people love to have it so." It's all clean dirt to them. Money, says Bacon is like muck, not good except it be spread. And it would appear that the inhabitants of the town referred to partly appreciate this remark; though it seems to one intimately acquainted with local affairs that they do not spread their money in order to manage and utilise the muck, but stick fast to the filthy lucre, while they display to the greatest possible disadvantage (of pedestrians) the varied qualities of the dirt, so that it may meet the eye at every turn!

But, with all gravity, we may venture to affirm that this is not merely a financial question. We shall not probably be far wrong if we regard it as one chiefly dependent on popular education. We can hardly hope to witness the full application of Sanitary Law till our working population is animated not merely by the craving for knowledge, but by an earnest desire to exhibit, in all the phases of social life, that right economy of force which we denominate "material justice."

In spite of the introduction of Science teaching among the people, the vast majority of our population remain—and will doubtless continue for some time to come—in a state of darkness respecting Sanitary Science. But the problem of improving public health consists not merely in the diffusion of knowledge, but in the application of moral and religious principle to the outward forms of life.

Clergymen, district visitors, and others might often do good, if well informed themselves, by pointing out violations of Sanitary Law to the poor. Ignorance itself is a powerful force, continuous in action and cumulative in its disastrous effects; the practical observance of law is, on the other hand, ever potent for good. A moderate acquaintance with physical law would often be of priceless value to those in the lowest walks of life. But, unfortunately, laziness and indifference are too often in contention with knowledge; the kindly warnings of Nature become, in time, a dead letter; familiarity has destroyed perception. To many of the inhabitants of our city lanes filth and disorder give no

feeling of uneasiness; the foulest of stenches scarcely produces a feeling of aversion. Yet we shall be doing great injustice to the poor in attributing such conditions entirely to their own action. Much of this mischief must come from conditions of life entailed upon them by the truly marvellous economy (to put the matter mildly) which builders, landowners, and architects have exhibited, with respect to space, in the arrangement and construction of dwellings.

V. ON TECHNICAL EDUCATION.

By Robert Galloway, M.R.I.A.

(Continued from vol. v., page 604.)

In the last article I stated that I would propose a scheme less costly and more efficient than the Department of Science and Arts' plan, prefacing the description of the scheme by showing what could be accomplished in Evening Science Schools, by describing what has actually been accomplished in Evening Science Classes in Chemistry. But before entering on these topics it appears to me desirable to notice one or two of the views put forward on Science teaching at the British Association, and Social Science, meetings, last autumn.

The first I will notice was that proposed by Mr. Lant Carpenter. The essence of his system, he stated, was the employment of a specially appointed Expert, who should go from school to school with *his apparatus, repeating the same lesson in each.* He said the system was conceived and elaborated during the last few years by the Liverpool School Board. In this he is totally mistaken; as far back as 1859 the respected Keeper of the Mining Records in the School of Mines, Mr. Robert Hunt, established this system in Cornwall under the name "The Miners' Association." He gave a detailed statement of the system, as to its working, &c., in 1868, to the Select Committee appointed by the House of Commons to inquire into the provisions for giving instruction in Theoretical and Applied Science to the industrial classes; and his evidence is fully set forth in the

Committee's Report, which was printed in that year by order of the House of Commons.

That system, in my opinion, would never in the long run aid in promoting the progress of sound Scientific and Technical Education: granted that the teacher when he first entered on his duties was not only thoroughly in earnest, but had the greatest enthusiasm possible for his task, how long would that earnestness and enthusiasm last? How long could he take that real and ardent interest in his pupils, which all true and effective teachers must take, in all the schools he visited? The teaching given under such a system must after a time degenerate into a mere matter of routine with the teacher; and an energetic man would try to escape from the thraldom by seeking some other employment more congenial to his altered views and wishes, and also more remunerative, for he would soon learn by experience that he could look for no advancement as a teacher, and therefore, if he continued to teach, it must be the same dreary round year after year, *repeating the same lesson in each*, until failing strength rendered him incapable of continuing it, and he would by that time have become unfitted for any other employment.

But what would be the remuneration for teachers of this class? The teacher in Cornwall received £100 a year at the time Mr. Hunt gave his evidence; but if we are to take the average amount paid by the Department of Science and Art to Science teachers as the STANDARD, it is evident that the scale of remuneration, for this class of teachers at least, is decreasing; for the average payment per teacher given by the Department in 1869 was £40, and in 1880-81 it was £24 13*s.* 11*d.*, being a decrease of more than 38 per cent. Such a teacher as Mr. Lant Carpenter describes, unless he got situated in a locality more than usually favourable in the cause of scientific education, or he had some means of his own, might end his days in the poorhouse. But Mr. Carpenter appears to be unaware that such a system has forced itself on many of the Science teachers under the Department, owing to the small and precarious sums they earn on the *result system*. In the last article it was shown that in some cases the remuneration the teacher of an experimental science received was less than 2¼*d.* per hour for the time he was occupied in preparing for and giving a lesson: this *lavish* remuneration, it will be observed, did not extend to the full hours of a working day, but only for the time he was occupied; whereas the Dock labourers in London, who are considered to belong to the lowest class of labourers in

that city, are paid at the rate of 5*d.* per hour throughout the entire working day, if they are constantly occupied, and the occasional labourers are paid at the rate of 6*d.* the hour.

Science teachers, like unscientific people, could not keep body and soul together on the rate of remuneration of £1 for twenty-eight lessons; they have therefore—in order to eke out what in some cases, after all has been done, will amount but to a bare subsistence—to teach in many schools, often also many subjects, and to devote an amount of time each day to teaching, which is directly in opposition to the system pursued in German schools. In the article in the February number (vol. v.) it was shown, from the Department's own Reports, that some of them taught in as many as *nine* different schools.

The *great decrease* in the average payment per teacher which has taken place within the last few years is, in my opinion, a most significant fact; it must be due, it appears to me, either to the employment at the present time of a class of teachers inferior to those employed in 1867, or it must be due to the departmental officials cutting down the earnings of the teachers, which—according to the late Sir Henry Cole, as shown in the January article (vol. v.)—was the chief, if not the sole, reason for instituting *the payment on result system.*

If the decrease in the average payment is due to the present teachers as a body being inferior to those employed in 1867, it indisputably proves, I think, that the young men, who have within the last dozen years or so been not only provided through the Department with a scientific education free of all cost, but have also been paid by the State—in the form of exhibitions, scholarships, &c.—varying sums for maintenance during the time they were studying, have not, after completing their studies, entered as teachers to any appreciable extent under the Department; otherwise, if the decrease in the average payment is not due to the cutting down of the teachers' earnings, but is due to the inefficiency of the teaching, it would prove that more efficient teachers were to be had before the Department took the training of them in hand than since that time.

The following sums appear in the Estimates for 1883-84 for exhibitions and maintenance allowances for those of the State-aided students who are sent to the Normal Science School, South Kensington :—

Exhibitions and Maintenance Allowances.

12	Exhibitions, at £50 a year, tenable for three or four years	£600
4	Scholarships, of £15 each	60
2	Scholarships, of £25 each	50
50	Teachers in training, at £1 1s. a week	1900
200	Teachers for short courses, £2 each ...	400
268		£3010

The plan which appears to have very generally come into vogue within the last few years, of forcing the student attending lectures on any of the Inductive Sciences to take down in his note-book, as completely and perfectly as possible, the *very words* the lecturer utters, and some time after submitting the copy to the lecturer or his assistant, and being questioned upon the lectures at stated times, either by a written or *viva voce* examination, appeared to be a method very much approved of at one of the two autumnal meetings I have already referred to; it is therefore deserving of notice.

The method of teaching the Inductive Sciences by means of Lectures, especially if they are not rendered catechetical, is a most irrational and delusive plan as far as the student is concerned, for the lecturer does not know how the instruction he is giving is being *mentally* received by his different hearers,—whether the words he utters remain passively in the mind as mere words, or, if not passively received as mere words, whether the mind is forming correct or incorrect ideas upon the subject lectured upon; the students might as well be "stocks of wood or stone," so far as the lecturer is aware how the instruction he is giving is operating on their minds.

The only advantage a lecture on an experimental science has over a book, the book having many advantages over the lecture, is this,—the student *sees*, or ought to see, the experiments performed at the lecture, whereas he only *reads* about them in the book; the difference corresponds to the difference between seeing a picture and simply reading about it. But with regard to such a non-experimental science as Geology, if it is not taught in the field, but only in the lecture-room, a standard work on the science well illustrated with diagrams is superior, almost in every respect, as a teaching agent, to such unpractical and unnatural teaching;

for a book is ever at hand to be consulted, and the student can peruse and refer to those portions of the subject which he does not at the first thoroughly comprehend; whereas the words uttered by the lecturer, if not understood at the time they are uttered, in most cases produce no after mental effect, ceasing even to be remembered as mere words.

Even as a method for cramming students for examinations the mere attendance on lectures has been found to be inoperative with the majority of students; hence the adoption of the system which has just been described, of compelling as far as possible the students to take down the exact words the lecturer utters; it may be more efficacious than the system it has superseded in cramming students, but what is it but the vicious system of rote learning carried out to its fullest extent? It leaves the intellect uncultured, for it forces, as far as it can, the student to be a mere passive recipient of others' ideas, and it prevents him, as far as can, from being an active inquirer and self-instructor. Even when students are not compelled to take down the exact words, they are very apt of themselves to fall into this rote system if the lectures are not made catechetical: the Examiners for University work in great towns have directed attention to this failing in their Reports; they have stated that the written answers on several of the questions taught contained not much more than a reproduction of what had been said by the lecturers; they were a repetition of formulated phrases, in place of an intelligent assimilation and reproduction of their general purport.

But what will be the condition of a student if he is taught by an ignorant teacher who adopts this rote system: would not the study of a good standard work on the science, even if the science were an experimental one, be far preferable to the utterings of such a teacher? And that there are such teachers abroad those who have been behind the scholastic curtain are aware. On one occasion a note-book, owned by a student who was being taught on this rote system, was placed in my hands; the part of the book to which my attention was directed had been signed correct by the *professorial* teacher: it consisted of a series of chemical equations, which were intended to describe the chemical changes which take place in the different forms of the galvanic battery; although signed correct by the Instructor every one of the equations was incorrect, and the errors were so glaring that they proved unmistakably the great ignorance of the subject which existed in the minds both of the teacher and the taught. If this sad lack of knowledge

is met with amongst teachers styled Professors, what can be or ought to be expected from teachers who are paid at the rate of £1 for twenty-eight lessons?

Unfortunately, in this land of educational endowments, ignorance is not solely confined to the teaching body; it extends sometimes to the examining body. In looking over examination papers it is not difficult to see that the questions set sometimes have been arrived at by the Examiner simply reading some book on the subject. I have met with one or more very curious instances of this, but to narrate them would carry me too far away from my present subject, and therefore I will resume the consideration of the lecture system.

A combination of practical work and thought is as necessary to obtain a knowledge of any of the inductive sciences as it is to obtain a knowledge of arithmetic; hence if any system of teaching is adopted for teaching any one of these sciences in which thought and work are not combined, information may, but knowledge certainly cannot, be acquired. The lecture system of teaching does not admit of the students combining work and thought; hence it cannot educationally be regarded as a method by which scientific knowledge can be acquired; nor is it rendered more efficient in this respect by compelling the student to write down the words the lecturer utters. To render lectures as efficient as it is possible to make them, the catechetical system must be adopted: by this system the attention of all the students remains unabated throughout the entire lecture, and by being converted from *passive* into *active* agents they take a great pleasure in the instruction given; and by being asked and asking questions all understand, as far as they can without experimenting themselves, the subject lectured upon. By adopting this method I have found students to be more deeply interested, if possible, in organic than in inorganic chemistry, whereas, by the professorial system of lecturing, as far as my experience extends, it is the branch of chemistry least liked. It is greatly to be regretted that the system of teaching by lectures is the prevailing system for teaching the Inductive Sciences, and that it is the system most frequently employed for teaching beginners; much more could be accomplished during the time the majority of students can devote to study if a more rational educational system were adopted.

In teaching evening classes in Chemistry, some years ago, I adopted a practical system of teaching from the first lesson onwards, which caused work and thought to be

combined; and I consider that results were obtained that have never been equalled—I will even go so far as to say approached—in evening classes, even in the very highest scientific schools, not only in the United Kingdom but in other countries, including Germany. It is necessary, in making this claim on the part of myself, not only to state the results obtained, but also other particulars in connection with these evening classes, so that others may be able to judge whether the claim I make is substantiated by what was accomplished by the students. And I think the facts I shall state will not be without interest to many at the present time, when so much is being said about Scientific and Technical Education.

Some teachers, I have no doubt, would be able to reply that they have also taught Chemistry practically from the very commencement of the course of instruction they gave. It is therefore necessary to show wherein the difference, as I think, exists, in an educational point of view, between the system pursued by others and the system I adopted, and in doing so I shall discuss the question as I would any other scientific problem. I think it will be admitted that I am not far astray when I state that if an elementary practical course be given, it is usually given on the plan set forth in Part I. of the small work by Mr. Francis Jones, entitled "The Owens College Junior Course." This course does not differ in any essential particular from that adopted by lecturers, with the exception that the student in the one case, and the lecturer in the other, performs the experiments. In neither case, as I think, is the course founded on true psychological principles; it is taught by these systems as it might have been, and was, taught when all that was known of the science of Chemistry consisted of little more than a number of isolated facts.

The first and great object in teaching any subject—whether classics, the deductive or inductive sciences—is the training of the intellectual faculties, in other words increasing and strengthening the reasoning powers; the second object is to furnish the mind with material upon which to reason, conjoining knowledge and the power to wield it, thus bringing, as it has been stated, all Nature under one sway. It will not be denied that the system of instruction which is the most perfect in training the intellectual faculties is the one which furnishes the mind in the best manner with material on which to reason; and if the teaching is of a kind that does not to any extent assist in training and strengthening the reasoning powers, the information

conveyed will not be made a part of the learner's own intelligence; he cannot therefore reason upon it, and therefore he will not be able to apply it.

Chemistry is more difficult to teach, from an educational point of view, than any of the other inductive sciences; it consists of such a multitude of facts without apparently much connection, and to fill the mind of the learner with any amount of these disconnected facts, which is the sort of teaching most usually adopted, is not the teaching best adapted for increasing and strengthening the reasoning powers, and, further, it does not furnish the mind with the scientific material in the best form on which to reason. Yet, from a wide and varied experience, I have found it is capable of being taught by a system which renders it a most efficient agent for training the intellect and storing the mind with material of the greatest interest for it to reason upon, and usefully to apply.

Instead of commencing the teaching of the science by showing that two elements, A and B, can combine together, and so going on through a series of combinations of the different elements, which is the plan usually followed, the students are taught by my system, *once for all*, by means of a few experiments, that all the elements are capable of uniting with one another; and they at the same time learn, by the aid of these experiments, that the *conditions* necessary to bring about their union vary, and also the phenomena which attend their combinations. This is a generalisation that I have found all, from 12 years old upwards, are capable of grasping, whereas the majority of students are bewildered when they are taken through the maze of combinations given in the course usually followed. By a similar generalisation they are made acquainted with the combination of compound bodies. The conditions which favour the decomposition of compound substances were next studied. The experiments in this part of the course were performed by each individual learner, each of them being *separately superintended and instructed*. They were taught at the same time the language of the science, as a language, by means of exercises; they thus obtained a complete mastery over it. By no other system is the student taught the language as a language, and without a knowledge of it he is without the instrument and nutriment of thought, and from ignorance of it the atmosphere in which chemical thought lives is wanting.

I have dwelt upon the teaching of the elementary portion of the Science at greater length than I shall the more

advanced portions, because I believe it is the portion of the Science which is the worst taught at the present time, yet it forms the foundation on which the further cultivation of the Science must rest, and therefore the system which most conforms to the laws of mental evolution ought to be the one followed, both for the purpose of rendering the Science as efficient an agent as possible in training the intellectual faculties, and for the purpose of promoting the more successful cultivation of it.

Qualitative analysis was the next branch of Practical Chemistry taught; in the previous course they had been made practically acquainted with the principles on which separation of substances from one another rests; quantitative analysis next followed, and finally chemical research. The Institution in which this instruction was given was a Government one, under the Department of Science and Art, called at that time the Museum of Irish Industry.

The amount of metal in most of the specimens of the ores of copper, iron, lead, and silver, and the assay of all the samples of coal employed in the manufacture of gas, and the estimation of the tannin in the different tanning materials, exhibited in the public museum attached to the Institution, were estimated by these evening students; the rest of the specimens were analysed by my day students. The researches carried out by a number of the evening students was published in various journals; I send a copy of one, "On the Comparative Value of the different Feeding Substances for Horses," to the Editor of this Journal, and I think he will admit it is as complete as it would have been if it had issued from the Laboratory of Sir J. B. Lawes and Dr. Gilbert. Sir Lyon Playfair, M.P., visited the Institution officially just as he was on the eve of terminating his connection with the Department. I asked him to visit my Evening Classes; he did so, and he stated openly to the students that he knew of no instruction of like excellence given in any other institution, and they were fortunate in having such a teacher.

I will mention two more facts, and will then pass on to other matters of interest. A young man in the Excise presented himself one evening, and told me he wanted to get into the Excise Laboratory in London, but he had been rejected in Chemistry at the Entrance Examination; he had one more chance, and wanted to be prepared. I told him I prepared no one to pass examinations; if I had I could have filled my department three or four times over; I taught, I said, the Science, and I left the taught to apply

it: he remained, and some six months after he competed again, and obtained the first place in Chemistry. Some little time after he had been in the Excise Laboratory, he, along with the other students, was examined by Dr. Hofmann; they had a written, a practical, and a *viva voce* examination: my old student got full number of marks in each. Dr. Hofmann inquired of him who first taught him Chemistry; Dr. Hofmann wrote to the Excise authorities and stated he had never met with a like case, and recommended them to grant him a special prize.

The last case I will narrate is a proof of the benefits the country might derive from the researches of evening students; the estimation of the tannin in the different tanning materials attracted the attention of English tanners, and some time after they formed a Society for the purpose of ascertaining the best tanning materials and the best practical method for estimating the tannin: one of the members wrote to me—we were strangers personally—asking me if I would let some of my students carry out the research, and they would give them £30. I was pleased, because it showed that strangers had confidence in the work of my students. I consented, but was unable to have it carried out, as a bitter persecution by permanent officials was commenced against me at that time; this was after Sir Lyon Playfair had left the Department.

I will now come to the class of students who attended these classes, and the cost. The students were of all classes and of all ages: there were doctors of medicine in practice, medical students, pharmaceutists, owners and managers of chemical factories, young men in smelting works and gas works, photographers, soap, manure, and soda-water manufacturers; and several of the younger students were able, from the instruction they received, to afterwards start chemical works of one kind or another. Many of them used to come from distant parts of the country. I found both day and evening students all apparatus and chemicals; and although I had large classes, both day and evening, I was able to accomplish this for £125 the year, including all the necessary expensive apparatus.

I believed I was accomplishing a good work, for it is the kind of education that is most required in Ireland. I therefore spared neither labour nor time; I was in the Laboratory from 10 a.m. till 10 p.m., with only an hour and a half interval, and then from 10 p.m. till 3 a.m. I was busy at work preparing lessons; it was during those late hours that my "Second Step" was written.

Yet it was when when this beneficial work was being more and more appreciated, the Science and Art Department determined to break up the Institution, and have, in place of this advanced technical instruction, a few popular science lectures delivered in another place. Gentlemen who had been students with me united with another gentleman to defeat this; a Parliamentary inquiry took place, and the late Sir H. Cole and Col. Donnelly were defeated, and the Institution was saved and re-christened. It is now called the Royal College of Science. For this I was never forgiven, at least by one party.

I have narrated these facts, and I shall when I come to show the necessity for a Minister of Education relate a few more, in order to give the public some real information about one of the most extravagant Government Departments of the day, which unless it is thoroughly reformed will strangle real scientific education in the future, as it has done in the past.

I must leave the description of my scheme for the next article.

VI. ON ANIMAL CHLOROPHYLL.

WHETHER chlorophyll is an exclusively vegetable product, or whether it is also elaborated by certain members of the animal world, is a question which has been not unfrequently discussed of late years without any decisive result. Recently, however, a series of researches on the chlorophyll of the lower animal forms has profoundly shaken the faith in a genuine animal chlorophyll. Morphological, physiological, and chemico-physical observations have shown, or seemed to show, that the chlorophyll-granules found in rhizopods, sponges, polypes, and ciliata are merely distinct vegetable individuals, Algæ, which inhabit the animals in question in a semi-parasitical manner, just as do Algæ and Fungi in lichens.

It need scarcely be said that, if such is universally and exclusively the nature of the chlorophyll observed within the bodies of animals, it is the great and certain feature by which animals and plants are distinguished from each other. This generalisation, cannot, however, as yet claim our acceptance, since Herr T. W. Engelmann ("Pflüger's Archiv. für Physiologie") has published certain observations which, to say the least, must render the above-mentioned assumption very questionable.

Some years ago Engelmann met with Vorticellæ whose tissues did not contain distinct granules of chlorophyll, but displayed a diffused homogeneous green colour as if from imbibed chlorophyll. As at that time the occurrence of animal chlorophyll was not questioned, he paid no further attention to these animalcules. Now, since the closer investigation of this colouring-matter has become of capital importance, he returned to the subject. He met with the necessary specimens only after long search, and in small numbers. He found upon *Vaucheriæ*, both living and dead, along with numerous colourless specimens of *Vorticella campanula* and *V. nebulifera*, a few Vorticellæ of a diffused green colour. In addition to their colour, these were distinguishable by the closer and more delicate annulation of their bodies, and by a greater transparence of their endoplasm. The green colouration was strictly limited to the ectoplasm. In recent specimens the colour was quite homogeneous. It was deepest in the so-called cuticle, and was wanting on the surface of the peristomal disc. The entire endoplasm and the long nucleus seemed to contain not a trace of chlorophyll. In different specimens, however, both the intensity and the distribution of the colour were found to vary.

The important question whether living chlorophyll—or at any rate a genuine, assimilating chlorophyll—was present was investigated by Engelmann by means of the so-called Bacteria-method. He added a small quantity of a liquid containing bacteria to a drop which contained a bright green *Vorticella* attached to a dead branch of *Vaucheria*. The latter was free from chlorophyll. The preparation was placed in the "dark box" of the microscope, and the *Vorticella*, by means of a suitable arrangement, was illuminated by a spectrum. After about a quarter of an hour a distinct accumulation of movable bacteria was seen round the *Vorticella*, whilst previously none or but few had been seen.

Without doubt these Vorticellæ possess the property, by

means of the diffused green colouring-matter of their ectoplasm, of evolving oxygen under the stimulus of light. Thus we have here the first undoubted animals which assimilate by means of a chlorophyll forming part and parcel of their own living structure, and not by the mediation of vegetable guests.

The fact that the bacterial reaction, as regards its slight intensity, may be compared with that of Infusoria, which harbour living chlorophyll granules to some extent, is explained by Engelmann by the consideration that the assimilating colouring-matter was present only in very small quantity. Further, the Vorticellæ themselves consume much of the oxygen liberated, so that they can throw off but little externally. The Vorticellæ, indeed, as the author has proved by a series of observations, have a great need of oxygen,—a fact which agrees well with the supposition that they can prepare a supply for themselves.

Herr Engelmann has instituted further experiments in order to decide whether the colouring-matter of these animalcules is identical with the chlorophyll of plants, or whether it is a peculiar and merely analogous chromophyll. To this end the assimilative energy in the various colours of the spectrum was tested by means of the "Bacteria-method." Its behaviour was found similar to that of the green colour of plants. The colouring-matter was next obtained in the form of globules, and examined with the spectroscope. There was found a well-marked absorption-band in the red, between B and C, and an absorption at the extremity beginning with F. These peculiarities testify for the identity of the colour of the Vorticellæ with vegetable chlorophyll. Finally a series of chemical reactions were applied, especially that of concentrated sulphuric acid. The results still confirmed the identity of the two colouring-matters.

It is to be regretted that the scarcity of green Vorticellæ put for the present an end to these researches. In the meantime the facts ascertained suffice to put an end to the doctrine of the exclusively vegetable nature of chlorophyll. Even if it could be proved that the pigment of these Vorticellæ was ultimately derived from a vegetable source the fundamental principle would remain unaffected. For the principle is that there are animals which by means of a pigment combined with their own living plasma, and not to be distinguished from the chlorophyll of plants, are able to assimilate under the stimulus of light exactly as do green vegetables.

It is not probable that the green *Vorticella* is the only

animal which assimilates by means of its own chlorophyll. Years ago the author believes that he has met with specimens of *Cothurnia crystallina* of a diffused green colour. It may also be asked whether the blue, brown, violet, and other pigments found in a diffused state in the ectoplasm of various Infusoria are not genuine chromophylls, comparable with the xanthophyll, cyanophyll, rhodophyll, &c., of the Algæ? Here is a wide field for research, and especially for the application of the Bacteria-method.

Is it not possible that in certain cases portions of animal plasma may have become differentiated "pseudo-Algæ,"—chromophyll-granules? Is it not judicious to pause before pronouncing every coloured corpuscle, occurring in an animal, resembling a small Alga and evolving oxygen in the light, to be a guest or inmate of vegetable origin?

ANALYSES OF BOOKS.

The Agnostic Annual, 1884. New and Revised Edition. London: H. Cattell and Co.

THIS pamphlet, both in itself and in the circumstances connected with its appearance, is a phenomenon worth the heedful notice of all readers. Its editor seems to have drawn up three questions and sent them round to a number of "distinguished scientists and honoured thinkers." The questions put to these representative men are:—"(1) Whether, in your opinion, Agnosticism is in accord with modern science? (2) What is its special relation to popular theology? And (3) Whether you believe it is destined to supplant religious supernaturalism?"

As far as we can glean from the text, certain of the parties thus interrogated—whoever they may have been—did not respond at all. Others seem to have answered, indeed, but requested the editor to "regard their communications as private." A third body in their replies made no such proviso, and their opinions have accordingly been published in the form of a symposium. The names of these gentlemen are Prof. T. Huxley, Pres. R.S., Mr. P. A. Taylor, M.P., Prof. F. W. Newman, Prof. Ernst Haeckel (of Jena), and Mr. J. Beal. In addition we find the opinions of "G. M. McC.," of W. Stewart Ross, Charles Watts, "Ignotus," W. B. McTaggart, W. Sadler, and "Julian,"—all recognised as advocates and champions of what is commonly known as Free-Thought.

Foremost stands the reply of Prof. Huxley. The President of the Royal Society here informs the world that he, some twenty years ago, invented the word "Agnostic,"—an instance, we may add, of his well-known propensity for the coinage of names. It was, he further tells us, "to denote people who, like myself, confess themselves to be hopelessly ignorant concerning a variety of matters about which metaphysicians and theologians, both orthodox and heterodox, dogmatise with the utmost confidence." He further tells us that "Agnosticism is honoured by especial obloquy on the part of the orthodox." He adds that he has "a sort of patent-right" in "Agnostic," and that it is his "trade-mark." Having thus asserted his claim to say what Agnosticism is and is not, he goes on to answer the three questions. "Agnosticism," he rules, "is of the essence of Science, whether ancient or modern. It simply means that a man shall not say

he knows or believes that which he has no scientific grounds for professing to know or believe."

It might here, perhaps, be asked by a doubter what is the exact meaning which the learned Professor attaches to the words "scientific grounds"? We have heard of eminent *savants* being compelled to retract their assertions, thus proving that their grounds for "professing to know" were not truly "scientific."

Turn we to the second question and reply:—"Consequently Agnosticism puts aside not only the greater part of popular theology, but also the greater part of popular anti-theology.

We venture here to submit that this second question is scarcely needful. If we are not, except on what are commonly called "scientific grounds," to say that we know or believe anything, not merely "the greater part of popular theology and anti-theology," but much more will have to be given up as devoid of a sound basis.

Thirdly, and lastly, Prof. Huxley has "no doubt that scientific criticism will prove destructive to the forms of super-naturalism which enter into the constitution of existing religions." He adds, however, as a saving clause, "There may be things, not only in the heavens and earth, but beyond the intelligible universe, which 'are not dreamt of in our philosophy.' . . . . Agnosticism simply says that we know nothing of what may be beyond phenomena." Might it not have been judicious here to add that we do not know phenomena save as changes in our own consciousness?

Such, then, are Prof. Huxley's opinions on Agnosticism, expressed with his usual force and clearness. As such they were printed and put forth to the world in the first edition of the "Agnostic Annual," wherein Prof. Huxley's name ranked foremost among the contributors. The first impression was quickly sold out, and the publishers of the "Annual" found it desirable to issue another edition. Accordingly Mr. C. A. Watts wrote to Prof. Huxley, under date November 16th, asking if he wished to "make any correction in or addition to" his communication? Probably to his surprise he received the following reply:—

"SIR,—I shall be obliged if you will inform me by what authority you have printed and published a *private* letter which I addressed to you, and have announced my name as one of the contributors to a publication *of which I never so much as heard* until I received a copy from you. My answer to your letter of the 16th inst. will depend upon your reply to these two questions. —Truly your obedient servant,

"T. H. HUXLEY.

"C. A. WATTS, Esq."

We must first call the attention of the reader to the words which we have italicised, and then refer him to the original letter

of Mr. C. A. Watts (of September 13th), in answer to which Prof. Huxley wrote his "communication." Mr. Watts wrote—"I have undertaken to see through the press a publication entitled the "Agnostic Annual," &c. As Prof. Huxley answered this letter, his assertion that he "never so much as heard of" the Annual becomes difficult to understand.

Secondly, as Prof. Huxley's communication was solicited with special reference to a work in course of preparation, the inference lay near at hand that it was intended for publication. Yet he did not take the very obvious course of marking his letter "private," though in the second communication of Nov. 18th he speaks of it as such.

Thirdly, it appears that though Prof. Huxley had received an advance copy of the Annual he allowed nearly three weeks to pass over without protesting against the publication of his "private" letter, and the announcement of his name as a contributor. This also is not easy to understand. A man who conceives himself aggrieved generally complains at once.

The correspondence proceeded until November 26th, when Prof. Huxley wrote to Mr. Watts as follows:—"My private (*sic*) letter, which you have published as a 'contribution' in your 'Annual,' does not contain anything of importance which may not be found in my published writings, so that the question as to the withholding of the publication of my views does not arise. Nor has your act caused me any further annoyance than that which every man feels when he is treated dishonourably."

Here the public will do well to note that Prof. Huxley does not in any way repudiate his answer to the three questions. On the contrary, he re-indorses it by proclaiming it substantially identical with what may "be found in his published writings." If so, why his anger? But the fact remains that what might be found scattered, and to some degree merely implied, in his works, is in his "communication" openly and clearly expressed and brought to a focus. As such it will fall into the hands of many who have never read his works, and of many more who may have dipped into them without getting a complete acquaintance with their teachings. As such it has not escaped the notice of the public press. As such, too, certain questions will be raised which might otherwise have been allowed to sleep. We only hope that we are not doomed, in consequence, to a second cataclysm, such as that brought on by the "Belfast Address."

Whether Prof. Huxley comes out of this contest as unscathed as we might wish a President of the Royal Society to do is a question which we may safely leave to our readers The effects of his last letter were not, however, what he perhaps expected. The publishers of the Annual—who, in deference to his position and merits, had previously determined, at a considerable sacrifice, not to issue the projected second edition—now resolved to

reprint the work at once, with the whole of the correspondence and with notes.

Mr. Stewart Ross combats the impression that "Agnostic" is a name adopted by certain "advanced thinkers that they may evade the popular odium of Atheist." He asks "Am I Theist, or Atheist? Define *Theos*, and I will tell you. The subject could now only be discussed in a tap-room."

Mr. P. A. Taylor, the tutelary genius, if we remember rightly, of the Anti-Vaccinationists, replies to the two former questions very much in the same manner as does Prof. Huxley. To the third he answers, "Agnosticism being, as I take it, purely negative, I do not see how it can supplant anything."

Prof. F. W. Newman, who declares himself not an Agnostic, but one who "believes that something can be known, and is important to be known, concerning God," says, as touching the first query, "If by Modern Science is meant Materialistic Science," he cannot imagine that any discord between its researches and Agnosticism could be possible. On the contrary, to Christian Theology it is a flat contradiction. That it will supplant Super-naturalism he considers doubtful. He points to the interest taken in "the ever-changing form of Spiritualism," and reminds us that in Ancient Greece and Rome a belief in astrology and necromancy increased as fast as disbelief in the popular religion."

Prof. Haeckel thinks that his Monism "agrees in all essential points with that philosophy of Nature which in England is represented by Agnosticism." He does not venture to decide the question how far Agnosticism is in a condition to influence the popular theology of the masses.

Mr. James Beal considers that the chances in favour of the acceptance of Agnosticism by the majority are for the moment small. He refers, in confirmation of his view, to "the revival and comparatively rapid spread of Theosophy."

"Julian" holds that Agnosticism is "quite as dangerous and full of mischief as theological faith."

May it not be said that the "Church Agnostic," as the President of the Royal Society terms it, carries in its bosom the germs of not a few divergent sects?

A question strikes us as regards the Introduction. We read there of "every liberal system," and of "uniting all liberal thinkers." Would not a definition, or at least explanation, of the term "liberal" be useful?

Solar Physics, an Almanack of the Christian Era. By A. H. Swinton. London: W. H. Allen and Co.

Have the sun-spots a definitely recurrent period of increase and decrease,—a maximum alternating with a minimum? So say astronomers not a few who have made it their especial business to watch for years the aspect of the sun. They maintain, further, that this alternate waxing and waning of disturbance in the solar photosphere occurs in a cycle of from ten to twelve years. Thus, in a table in the book before us, we find the years 1860, 1870, and 1882 laid down as those when sun-spots were most numerous. At the intermediate dates, 1867 the spots were fewest. Here, then, there can be no scope for argument. Nothing but prolonged observation can show whether the spots have a period, and, if so, of how many years does it consist?

We come now to the second question: admitting the cyclical character of the sun-spots, and ascribing to them—provisionally at least—a period of from ten to twelve years, we ask can a corresponding cycle be traced in any terrestrial phenomena? Many observers here reply in the affirmative. They trace a periodicity in the occurrence of earthquakes, tempests, floods, severe frosts, heat-waves, &c. Not less is the organic world said to be similarly affected; inroads of destructive insects, pestilence, cattle-plague, diseases in crops, commercial depressions, wars, nay even social and moral phenomena, such as outbursts of fanaticism, "revivals," political "reforms," and the like,—all, it is asserted, display the same periodicity. From an *à priori* point of view there is here nothing to be sneered at. To pronounce it impossible for one series of phenomena to be causally connected with another is extremely rash, until close observation has proved the contrary. But we cannot detect a cyclical succession in earthquakes, floods, and tempests without collecting notices of their occurrence. Hence we fail to see anything ridiculous in the author's habit of cutting out from the morning papers, and assorting all allusions to earthquakes, floods, general meteorology, and corn statistics. If he perseveres in such a course he will obtain the facts and their dates; he will be able to find, *e.g.*, whether such convulsions of Nature are more plentiful in years of maximum or of minimum sun-spots, or whether the two classes of phenomena have any apparent connection at all. It would, of course, be preferable if Mr. Swinton had a legion of agents distributed over the globe, each duly equipped with seismometer, rain-guage, anemometer, &c., and at once sending in to some central station the results of his observations. But in default of such an organisation, which it might exhaust the resources of a nation to maintain, the next best method is to avail himself of the public press.

The question whether the subject is in itself worth examina-

tion can scarcely be raised. If the sun-spots follow a demonstrable cycle with which the above-mentioned terrestrial phenomena coincide, the study of solar physics assumes the highest importance. By its means we may foresee whether any given year will be marked by many or by few earthquakes and cyclones, and we may in some respects modify our conduct accordingly.

In this sense—as far as prediction of the weather is concerned—Mr. Swinton's tables are at least an attempt at "an almanac of the Christian era." Even if, on further inquiry, the apparent connection between the sun-spot cycle and the earthquake cycle should not be capable of demonstration, the inquiry is not merely legitimate, but laudable. A negative result is vastly preferable to uncertainty. Hence we must pronounce the subject of Mr. Swinton's studies, the method he has embraced, and the purpose aimed at, all rational and worthy.

There are doubtless many cases where the theory of sun-spot cycles throws no definite light upon the course of the weather. More than a cycle has passed over since we have had a really genial, productive season. The maximum and minimum epochs have alike failed to bring us relief. From some instances we might conclude that the years immediately following the minimum should be the best. Thus 1867 was marked as a minimum. It was followed by 1868, the hottest and driest summer of recent times, with the finest wheat-crop both as to quantity and quality. The two following seasons, 1869 and 1870, though not equal to 1868, were yet fine and bountiful. In 1870 apples were so plentiful in many parts of England that they were thrown in despair to the pigs. But if we turn to the next minimum, 1878, we find a different tale. Its successor was "cruel '79"—perhaps the most disastrous year for the farmer on record.

Perhaps the greatest difficulty which has to be overcome, in establishing weather- and harvest-cycles, lies in the fact that different parts not only of the same hemisphere, but even of the same continent, may have seasons very different in character. Thus during the summer of 1882 cold and wet devastated Britain, France, Switzerland, Italy, and Germany. At the same time Hungary, Russia, and Turkey enjoyed a warm, dry season, and a bountiful harvest.

Hence it would seem that a plurality of causes must co-operate in determining the character of the weather. A further point to be considered is that the harvest is little affected by the total rainfall of the year or the annual mean temperature. It may be said to be, in England at least, directly as the temperature, and inversely as the rainfall of the summer months. Mr. Swinton makes the admission:—"It does not therefore appear that we must infer that all the great frosts of history recapitulated by Mr. Walford as happening between 401 and 1874 invariably closed in the cold years. Perhaps at the sun-spot periods we

have southerly winds with open winters in this island." No one will deny that the seeming caprice of the wind is a powerful factor in the character of a season. If the "polar current" sets in strongly in the first two weeks of January we can scarcely escape frost and snow. If the equatorial current maintains the upper hand at this critical epoch we have a mild winter. But by some curious circumstance, not as yet understood, southerly and south-westerly winds in summer are not always mild. In 1879 the worst fits of summer cold were brought by south-westerly winds.

Mr. Swinton's researches on the periodicity observed in the visits of certain species of insects have repeatedly attracted our notice. In the work before us he foretells, or at least indicates as probable, that in the coming year the larger and finer Sphinges, such as *C. Nerii*, *S. Convolvuli*, &c., may visit our shores along with certain rare butterflies, such as *V. Antiopa*, *Pieris Daplidice*, *Colias Edusa* and *Hyale*, &c. It is interesting to notice that during the past summer specimens of the two latter insects appeared on our southern coasts, possibly heralding a swarm to arrive in the next season.

We should recommend Mr. Swinton to concentrate his attention especially upon cyclical phenomena in the appearance of animal species. It is an interesting fact that for the last two seasons herring and cod have been exceptionally scarce and poor on the Norwegian coasts. This winter herrings have appeared in great force at the Lofotens, and it is hoped that the cod will follow their lead.

Natural Philosophy for General Readers and Young Persons. Translated and edited from Ganot's "Cours Elémentaire de Physique," by E. Atkinson, Ph.D., F.C.S. Fifth Edition. London: Longmans and Co.

This book is sometimes confounded with a larger work by the same author and editor. The volume before us is, however, not a mere abridgment of Ganot's "Eléments de Physique," but a version of his "Cours Elémentaire de Physique." It is specially intended for the general reader who, in these days, cannot well afford to remain devoid of an accurate, even though sketchy, knowledge of physics. Its claims upon the attention of the student are shown by the statement that it may be approximately taken to represent the amount of knowledge required for the matriculation examination of the London University. It is also adapted for the upper classes in schools. Its suitability for these purposes may be fairly judged by the fact that a fifth edition has

been called for. The editor has made such additions as the course of scientific discovery demands, but without otherwise modifying the character of the work.

It may perhaps seem presumptuous to criticise any statement in a work of such general accuracy, and so justly approved of by teachers of physics. The following passage strikes us, however, as being open to exception:—"The action of a *noise* (as contra-distinguished from *sound*) upon the ear has been compared to that of a flickering light upon the eye; both are painful, in consequence of the sudden and abrupt changes which they produce in their respective nerves." Now it seems to us that musical sounds are more distressing to a person who has any engagement other than listening to them than are most noises. We speedily get accustomed to the roll of street-traffic, to the rushing of a river, or the sough of the wind in the trees; but who can get inured to the tolling of church-bells, or to the strains of a piano or an American organ in the next house?

It may perhaps be asked why Dr. Atkinson retains the term "Natural Philosophy," in place of its more precise modern equivalent Physics?

How to Arrest Infectious Diseases. By EDGAR BARNES, M.D. London: J. and A. Churchill.

IT is in these days permissible to seek for the reduction—and, if possible, for the extinction—of some diseases, whilst others appear to be like water companies, "vested interests" sacred in the eyes of John Bull, and not to be restricted or interfered with. Fevers, smallpox, cholera, &c., have monopolised the title "preventible diseases," and the attention of sanitary reformers.

From this somewhat limited point of view the little book before us is a clear, intelligible compilation. It aims at serving the general practitioner as reminder of "minute details, familiar enough but apt to evade the memory just when they are wanted." As such the work is of considerable value; but it is also intended to be placed in the hands of any intelligent non-medical man as a guide to him in protecting his family from the ravages of these diseases. It is undeniably well calculated to serve both these purposes.

The author opens his subject by explaining the terms *infection* and *disinfection*. On the exact nature of infection he does not find it necessary to speculate, and uses the expression "disease-germs" merely in a provisional sense, without pledging himself to any theory as to their constitution. He describes the ordinary invisible dust suspended in the air, and enumerates its constituents as actually determined by microscopic and micro-chemical

research, italicising those calculated to be the causes, or at least the bearers of disease.

In the second chapter we find an account of the precautions to be taken during the progress of infectious disease. In boarding-schools he recommends, on the attack of fever, &c., not an immediate breaking up, and the consequent possible dissemination of the disease, but to divide, if practicable, the sufferers from the healthy in a separate building.

The author seems to favour aërial disinfectants. He admits that "by their use you cannot make an infected atmosphere pure and safe without rendering it destructive to the sufferer as well." Yet he holds that an atmosphere impregnated as far as practicable, *e.g.*, with chlorine, is unfavourable to the activity of disease-germs. He protests, most justly, against the selfish—and in fact criminal—practice of removing convalescents *whilst still infectious* to the sea-side. He shows that every such case may infect a couple of cabs and a railway-carriage, and the lodgings which the convalescent has occupied are rendered unsafe—perhaps even deadly—to succeeding occupants. This, indeed, shows some of the mysterious channels through which infection may reach the inmates of houses in which all sanitary precautions are duly observed.

We come next to the instructions for the management of special diseases.

The statistics of the London Smallpox Hospital show some very curious results. Whilst 35 per cent of the deaths were those of unvaccinated persons, and 23·57 of persons said to have been vaccinated, but having no visible cicatrix, 19 per cent *had previously had smallpox*, whilst only 14·93 per cent had been vaccinated. Hence it would appear that vaccination is a more efficient safeguard even than a former attack of smallpox! How will the Anti-Vaccinationists explain these figures?

At the same time another statistical table on the opposite page requires a little explanation. We read there:—"Vaccination optional, 1847—53, annual death-rate from smallpox per million 305." But optional vaccination had been practised for many years previous to 1847. What was the death-rate then?

Among disinfectants we find mention of ozone,—a doubtful agent to be dealt with by the general public.

Under the head of Legislation the author pleads for the compulsatory notification of disease. He would, however, throw the *onus* not on the medical attendant, but on the head of the household. Such a measure would be exceedingly unpopular, and we doubt whether the benefits derived by the community would be at all commensurate with the obvious hardships which it would involve.

Dr. Barnes has carried out his plan as completely as was possible within the scope of some eighty small pages, and his treatise will be a boon to no small part of the community.

On some Improved Laboratory Appliances for conducting many Chemical Operations at the same time, and hastening the Completion of several of them. By ROBERT GALLOWAY, M.R.I.A., F.C.S., and F. J. O'FARREL, M.R.I.A., F.C.S. (Reprinted from the "Philosophical Magazine."

THE authors of this memoir lay down the proposition that "it requires a greater expenditure of time and labour to arrive at results in Chemistry than it requires to attain like ends in any other of the inductive sciences." Without absolutely accepting this view, we may freely admit that the time and labour so expended are exceedingly great, and that any improvement tending towards this end must render the task of research lighter and easier. We are told, as an instance of what such inventions may practically signify, that if Liebig's apparatus for quantitative organic analysis had been devised before Chevreul began his examination into the constitution of fats, he would have been enabled to save seven of the fourteen years which he devoted to this investigation. Seven years of the working time of a man like Chevreul are no trifle.

Messrs. Galloway and O'Farrel remark that "at the present time a great deal of labour is involved, and time wasted, by having to set up in a laboratory, where a variety of work is going on, so many distinct pieces of apparatus, each requiring to be started separately. For obtaining the indispensable distilled water, a special still and heating apparatus is generally set apart for the purpose. Distillation under diminished pressure is another distinct, and frequently troublesome, operation. If a filter-pump is employed to hasten filtration, the pump is solely devoted to that purpose. Then there are the open and closed water-baths, each distinct and requiring a separate heating-apparatus; and for heating and drying substances above 100° C. the methods are most inconvenient. But of all the slow methods, that of evaporating by means of the ordinary air-pump and absorbing the water as it evaporates by means of sulphuric acid is, we think, the slowest. The appliances that are in use generally for forcing steam, air, or gases over substances, are, to say the least, inconvenient.

"By the aid of a small general air-pump connected to a little stationary engine, and ordinary or superheated steam, all these operations can be carried on at the same time with these appliances excepting the one last described: for this operation there is required, in addition to the air- or suction-pump, a compression-pump in connexion with the engine.

"In carrying out some researches recently we found great convenience and saving of labour and time in employing a general air-pump. This has led us to devise appliances for the

extension of our plan to a larger number of chemical operations, which we will now describe, as we think the arrangement will be found very useful not only in educational laboratories, but also in pharmaceutical laboratories, especially in the preparation of delicate organic compounds."

We cannot give a description of the authors' appliances without the accompanying illustrations, but we feel convinced that they will be widely useful in the laboratories of colleges, and in those of manufacturing establishments where large numbers of determinations are always in progress, and where economy of time is a matter of supreme moment.

Journal of the Royal Microscopical Society. Containing its Transactions and Proceedings, and a Summary of Current Researches relating to Zoology and Botany (principally Invertebrata and Cryptogamia), Microscopy, &c. Edited by FRANK CRISP, LL.B., B.A. Series 2, Vol. III., 1883, pp. 972. Published for the Society. London and Edinburgh: Williams and Norgate.

THE editor of this Journal and his able staff of assistants have again been unsparing in their efforts to place before English readers a summary of everything of importance relating to the microscope and those branches of Science needing magnifying power for their investigation, which has transpired during the past year both at home and abroad.

The papers forming the "Transactions" of the Society are only seventeen in number, but all of considerable value: the discussions following their reading have been reproduced from the short-hand writer's notes, and from the important matter elicited from those familiar with the subjects of the various papers form a valuable supplement to the more formal Transactions.

The fact of the original papers being so few in number is no fault of the Royal Microscopical Society, but rather the natural result of its work during the past forty-four years of its existence. In 1828, only twelve years before the formation of the Society, there were only four achromatic microscopes in London, and during the earlier years of its existence microscopes and workers were still few in number; and papers on microscopical subjects were rarely or never admitted to discussion at the older societies. At one of the early meetings of the Quekett Microscopical Club the late Dr. Lankester (then president) stated that he recollected the time, not very far back, when being known to be engaged in the study of minute anatomy would seriously prejudice a medical student's examination. The Microscopical Society and its earnest

members, however, worked steadily on; the late John Quekett laid the foundations of Histology as a science, and the result has been that at the present time the microscope is a necessary tool in most branches of scientific enquiry, and the papers which could only get a hearing at the Microscopical Society are now eagerly sought for, and find a place in the Transactions of numerous other bodies.

As usual, translations from foreign sources have been given at considerable length. An important paper on "Practical Processes in Vegetable Histology" occupies no less than twenty-two pages, and gives valuable information on a subject on which but little has as yet been published in the English language,

The summary is divided into three sections—Zoology, Botany, and Microscopy. The first two are subdivided into several sections, the latter into two; the first treating of new and improved instruments, the latter on various processes of collecting, mounting, and examining objects.

The current literature of the microscope is tabulated,—a matter of great importance, saving much weary searching among the ever-growing mass of modern periodicals.

As to rendering the large volume of the Journal accessible, no trouble has been spared; the table of contents and the alphabetical index are both of ample proportions and well arranged, a matter very often neglected, and detracting much from the value of otherwise good books of reference.

The whole book reflects the usual liberal spirit of the Royal Microscopical Society, always ready, as are most of their Fellows individually, to help other workers. No less than eighty-four societies, home and foreign, instituted for kindred objects, have been affiliated through the *ex officio* fellowship conferred upon their presidents; these, with the numerous eminent scientists who have received the compliment of honorary fellowship, extend the influence of the Royal Microscopical Society throughout the civilised world.

Longman's Magazine. No. XIV. December, 1883. London: Longmans and Co.

We may notice in this issue a paper by the Rev. J. G. Wood, entitled "The Mole at Home, and containing a very fair account of the anatomy, as well as of the habits, of the "little gentleman in black velvet,"—once enthusiastically toasted at Jacobite meetings. The following passage, however, is scarcely happy:—"The instinct coincides with the structure, so that we never see a mole trying to fly in the air, a bat trying to dig a burrow

in the ground, or a whale attempting to walk on land." Would it not have been more satisfactory to have read "the habits follow from the structure"?

The fact that moles can swim is by no means generally known. The author quotes a letter from a Mr. Clark, who, whilst justly protesting against the extermination of the rook and the mole, pleads for the sparrow also. If he would shoot and open every sparrow he comes across, he would find that during the greater part of the year these little pests feed not upon destructive insects, but upon grain and fruits. This, joined to the war which they wage against the harmless and useful swallow and martin, should surely settle their doom.

"The Modern Nebuchadnezzar," by A. H. A. Hamilton, is certainly a psychological curiosity. The author tells us that he met in Italy with a self-exiled Englishman who, according to his own account, had as a punishment for his vices, especially cruelty, been transformed into a horse, though retaining a clear recollection of what he had been. After seven years he was suddenly restored to human shape. He expresses the opinion that "experiences similar to his own have been the lot of many human beings." It need scarcely be said that the alleged phenomenon bears no resemblance to the "re-incarnations" of which the Occultists tell us. Nor can the hero of the story be fairly likened to Nebuchadnezzar, who is not stated to have lost the human form during his alienation. If such transformations are not uncommon the world is evidently a theatre of anarchy.

This fevered tale is not free from the poison which writers of fiction, on the principle of the blind leading the blind, so commonly instil into their more ignorant readers. The man-horse is put into the hands of a veterinary surgeon, who, he says, "lanced me and blistered me, and almost vivisected me." Here it is falsely insinuated that the pain inflicted upon animals under physiological examination is something altogether exceptional in its intensity. Even the Bestiarian leaders know better than this, though it suits their purpose to hoodwink the public.

Among the strange utterances to be found in this story is the assertion that "many persons have been placed in a position similar to mine, but the power of speech has been allowed to them only in a very few instances. Some of these are recorded in the early history of Rome, but the case of Balaam's ass is perhaps the best authenticated."

No. XV. January, 1884.

In a somewhat flippant article on the "Decay of the British Ghost" a Mr. F. Anstey, though evidently acquainted with the Psychical Research Society and its doings, and presumably not ignorant of the existence of Spiritualism, Occultism, and Theosophy, strangely misinterprets the tendencies of the time. The

British ghost is evidently reviving! Would that certain other features of bygone days, such as the absence of worry and a fair share of national prosperity, would follow his example.

The "Ancestry of Birds" is a fair account of the development of birds from reptiles, though containing little that is novel in fact or original in generalisation. It might, indeed, be asked whether the reptiles are "at present a small and dying race"?

The "Clerk's Tale" is an instance of life preserved by a hallucination or an apparition, and contains due reference to the Psychical Research Society.

The Mason College Magazine. Vol. I., No. 9. December, 1883. Birmingham: Cornish Bros.

We regret to find that the conductors of this interesting little magazine have found it advisable to reduce its yearly issues from nine to six.

Prof. Poynting's Address delivered at the Mason College takes up a note which we hear everywhere with satisfaction, but which, so far, leads to nothing. He justly likens the English system of teaching and examining "to the charging of secondary batteries which only give back what was put into them, with some inevitable loss of leakage. He thinks that in a true University training students should learn how to arrive at results by their own methods. This is what every man of sense, unless interested in the present system, thinks and says. But we do not rise to the emergency, and once for all tear the examination system to pieces and trample it under foot. We allow it to go on, wasting public money and stereotyping our youth into general incapacity.

Some remarks on "Duty" are, with the exception of one sentence of three words, very appropriate.

"Chemistry in the West Indies" is interesting, and might easily be rendered more so. It is to be hoped that the author will continue his contributions.

In a discussion on "Popular Narcotics" a medium view was adopted. They were pronounced not necessities, but at the same time not dangerous poisons.

Another interesting debate is reported, on the proposition "That it is desirable to agitate against the tyranny of fashion." The majority of votes was in the affirmative, though one of the speakers committed himself to the sentiment that it is "well to agitate against everything!"

The Science Monthly, Illustrated. Vol. I., No. 3, January, 1884. London: D. Bogue and E. W. Allen.

It is easy to see why this Journal has been sent to us. The Editor is not gratified at our comments on an utterance in his "Table Talk" for November anent the Anti-Vivisection hubbub, and he propounds it as a "riddle" to find the occasion for our remarks. This riddle may be easily solved by a reference to his own words:—"I hope (*sic*) the *Champion* may be able to compass this feat (*i.e.*, to prove that Vivisection is wrong): but one cannot help thinking that the *Zoophilist* takes the best ground after all, in contending that the practice is reprehensible because it is possibly useless and certainly cruel." In thus writing our contemporary, so to speak, formally hoists the Bestiarian flag.

Apparently in consequence of his expression of sympathy two works have been sent him from the enemy's camp for review, and they are noticed—one of them favourably—in the issue before us. We note, further, an article on "Experimental Physiology," the spirit of which is certainly not hostile to the agitating fanatics. The question is put, among others, "If Experimental Physiology be a legitimate branch of human inquiry, what ground is there for limiting its application to the brute creation? Might we not equally well ask, "If killing animals for food be legitimate, what ground is there for limiting its application to the brute creation?" Similar queries might be raised concerning compulsatory labour, castration, and other "cruelties" which man inflicts upon the brute creation. Until the Bestiarians are prepared to show that the infliction of pain and death upon animals is less legitimate in pursuit of knowledge than in pursuit of "sport," convenience, gain, luxury, &c., we hold that they have no logical right to say another word. But this task they never even attempt.

Woods and Forests, Landscape, Covert, Park, Arboretum, and Nursery. A Weekly Illustrated Journal of Forestry, Ornamental Planting, and Estate Management. No. 2.

We are, both by heredity and by early association, worshippers of trees. We honour those who plant and protect them, and we loathe those who deface or needlessly destroy them. We need scarcely, therefore, say that a paper like the one before us, which recommends and enforces tree-planting, and gives practical instructions as to how it may be best effected, has our hearty good wishes.

CORRESPONDENCE.

*** The Editor does not hold himself responsible for statements of facts or opinions expressed in Correspondence, or in Articles bearing the signature of their respective authors.

PROFESSOR TYNDALL AND MATERIALISM.

To the Editor of the Journal of Science.

SIR,—Until very lately Professor Tyndall has been universally held to be a Materialist. As an instance of the manner in which he has been spoken of I take a few lines which you, in your October issue (p. 614), quoted from a Lecture delivered by Dr. Usher at Ballaarat. The lecturer is reported as saying—"It is incomprehensible to me how men . . . nevertheless reject a Designer, as everyone must acknowledge who reads Tyndall's Belfast Address between the lines—rank Atheism, thinly veiled throughout." Yet an American journal quotes him as saying—"If asked whether Science has solved, or is in our day likely to solve, the problem of the universe, I must shake my head in doubt. Behind, above, and around us, the real mystery of the universe lies unsolved, and, as far as we are concerned, is incapable of solution. The problem of the connection of the body and the soul is as insoluble in its modern form as it was in the pre-scientific ages."

Here, therefore, the existence of a soul—a distinct entity requiring to be connected with the body—is admitted. It may further be said that its immateriality is conceded. Why else should its connection with the body be recognised as an especially difficult problem?

But, if the word "soul" is here to be taken in its ordinary sense,—and Prof. Tyndall makes no stipulation to the contrary,—how are we to "discern in Matter the promise and the potency of all terrestrial Life?" How, again, are we all, "like streaks of morning cloud," to "melt into the infinite azure of the past?"

The quotation proceeds:—"There ought to be a clear distinction between Science in the state of hypothesis and Science in the state of fact, and, inasmuch, as it is still in its hypothetical stage, the ban of exclusion ought to fall upon the theory of Evolution."

Yet in the Belfast Address Prof. Tyndall pleads ably and earnestly for this very theory of Evolution, and even speaks of "the

doctrine of the survival of the fittest, which in our day, on the *basis of positive knowledge*, has been raised to such extraordinary significance."

What, then, is Professor Tyndall,—Materialist or Spiritualist, Monist or Dualist? The trumpet or the fog-horn gives an uncertain sound.—I am, &c.,

VARRO.

OBSERVATIONS IN NATURAL HISTORY.

To the Editor of the Journal of Science.

Markings of the Lion.—Ten years ago I saw lion cubs, at Wombwell's show, with leopard spots visible by side light, like "shot-colors."

Squirrels.—Grantley Berkeley found the squirrel an enemy to pheasants and partridges; and although it is, as you say, "a tree rat," it does not confine itself to trees. In winter I found one in a hedge, far from trees, above 12 feet high; and, near Barmouth, I saw a white-tailed squirrel leap from stone to stone along a brook with greater activity than a rat, and I suspect he was looking for *fish*; he was so eager that, as I stood motionless, he passed almost under my fishing-rod. The white-tailed squirrel is rare, and I never saw another.

Fireflies.—Leigh Hunt, in his "Autobiography," speaks of the brilliance of fireflies in Italy, and says that they are not noticed by Greek or Latin writers; and he thinks they are first mentioned by Dante, and that they may have been brought from America. Is this correct?

Hybernation.—Must not the mammoth have hybernated?

HUGH BROWNE.

Nottingham, January 4, 1884.

TAPE-WORMS AND HERMAPHRODITES.

To the Editor of the Journal of Science.

SIR,—Professor Huxley, in his Letters to Working Men (Hardwicke, 1863) "On our Knowledge of the Causes of the Phenomena

of Organic Nature," says, "The tape-worm exists in the human intestines, so that the fewer there are of men the fewer there will be of tape-worms, other things being alike. It is a humiliating reflection, perhaps, that we may be classed as direct helpers to the tape-worm; but the fact is so. We can all see that if there were no men there would be no tape-worms" (p. 122). Whether or not it is received as a biological fact, that the tape-worm is alone incidental to man, I do not know. Clearly the inference to be drawn from the above quotation is, that such is the fact. In opposition to this opinion I have an observation of my own to offer, *viz.*, that the tape-worm is present with other animals besides man. I had a bull terrier which, when about nine months old, was sadly troubled with worms, usually maggot-like creatures. On three several occasions, under my notice, the dog voided tape-worm from 6 inches to a foot in length.

On another question in Physiology there appears to be a fog—*viz.*, as to the creatures which are hermaphrodite. The snail, I believe, is instanced, but not the common earth-worm. This is an hermaphrodite; each worm impregnates the other by a double process common with both. This fact can be ascertained by any person interested in the subject in the evenings of August. The worms may be seen lying heads towards tails (I have seen them in hundreds); on being separated the distinct organs may be seen. Whether this fact has been observed by others I do not know; I find no notice of it in books. I do not suppose it is possible that my dog and my worms are isolated facts.

Whether the facts are new or not I have thought them worth recording, and hence the reason for my troubling you.—I am, &c.,

S. Billing.

[There are at least three species of tape-worm which occur in the dog,—*Tænia serrata*, *T. echinococcus*, and *T. marginata*. These are distinct from the species inhabiting man. The entire order to which the earth-worm belongs is hermaphroditic.—Ed. J. S.]

NOTES.

THE following singular advertisement appears in the "Deutsch-Kroner Zeitung" of December 11th:—"Magpies shot between December 24th and January 6th are used for a remedy against epilepsy. The undersigned, with whom this medicine is prepared, will be greatly obliged to everyone who will send him at that time as many magpies as possible, provided that they have been shot, and not killed by poison or caught in traps.—Castle Tütz, Dec. 5, 1883.—Signed, THEODOR, Count Stolberg.

[We feel strongly tempted to consider this advertisement as due to a survival, or rather a recrudescence, of superstition.]

Prof. Edlund controverts the generally-received statement that a vacuum—or highly rarefied air—is a non-conductor of electricity. He maintains that the obstacle to the passage of the current lies in the resistance of the electrodes, and disappears entirely with their suppression.

According to Dr. S. B. Higgins ("American Naturalist") the copperhead snake (*Ancistrodon contortrix*) always strikes at the ankles of its enemy, in contradistinction to the Crotalidæ, which aim higher.

Dr. Hyades, of the late French Expedition to Cape Horn, reports that rabies are unknown among the dogs of Tierra del Fuego.

M. Rochefontaine ("Comptes Rendus") announces that cholera, smallpox, typhoid fever, and anthrax are by no means uncommon among the coppersmiths of Villedieu.

According to Dr. Stone the human body is a far better conductor of electricity when suffering from paralysis than when in a state of health.

A fungus (*Aspergillus glaucus*) flourishes well in the solutions of the alkaloids, such as morphine and atropine.

Dr. Landwehr ("Zeitschrift Phys. Chemie") has obtained a true gum from mucine and metalbumen. It is capable of fermentation, and by boiling with dilute acid is converted into a sugar which reduces copper oxide.

The Society for Psychic Research proposes a so-called "ghost-census." The Society is seeking for cases "where some marked event happening to a person at a distance has coincided with the apparent perception of that person's presence or with a dream of him."

Trout-breeding, as it appears, is fairly successful in New Zealand. Mr. Pillans, of Otago, has now in his nursery tanks about 6000 yearlings and 250 two-year old fish, all thriving.

The "poor humanist" ought to feel gratified with the Peerage offered to the Laureate, a man unfriendly to Science and of Bestiarian propensities. During the lifetime of the present generation four Peerages have been awarded to "letters,"—to Science none!

According to the "Electrical Review" Mr. G. G. Rockwood has succeeded in photographing the waves of sound. What say the "Substantialists"?

According to Sir R. W. Rawson the measurements of the Anthropometric Committee show that from the top of the social scale to the bottom there is a gradual descent in stature, chest-girth, weight, and all the elements of strength.

A submarine observatory is, according to the "Petit Niçois," about to be constructed at Nice; the submarine flora and fauna illuminated, if necessary, by powerful electric lamps.

Prof. Von Fleischl ("Chem. Physik. Gesell. zu Wien") concludes that the retinal rods supplied by one and the same nerve-fibre do not form an anatomical group, but stand intermixed with rods supplied by other nerve-fibres.

Mr. W. K. Brooks ("American Naturalist") argues that the germ-cell is the persistent and the sperm-cell the progressive element in the formation of a new being.

M. Auguste Charpentier ("Comptes Rendus"), in course of researches on the perception of differences of illumination, shows that the *differential fraction* (the excess of the illumination of the object over and above that of the back-ground) is by no means constant.

M. V. Jodin ("Comptes Rendus") has succeeded in cultivating plants in solutions of putrescent organic matter.

According to the "Medical Press" the water-supply of Newcastle is supplemented in dry weather by fluid pumped up from the Tyne, below the point where the sewage of Hexham is discharged into the river.

R. Billwiller, from anemometrical observations conducted simultaneously on the Santis, 2467 metres in height, and in the valley, finds that calms are very rarely to be met with except on the surface of the earth.

"Science" recommends that authors who re-publish, in a separate form, memoirs which first appeared in the Transactions of Societies, &c., should be careful to retain the paging of the original.

M. J. Thouclet has studied experimentally the velocity of currents of air or water capable of holding mineral particles in suspension.

According to the "Comptes Rendus" the extraordinary phenomena accompanying sunset and sunrise, in November and December last, have been observed at Lyon, Rambouillet, Valencia, and Christiania.

Dr. Carter Moffat, in a Lecture delivered recently at Glasgow, maintained that the presence of hydrogen peroxide in the air and dew of Italy had some connection with the beauty of the Italian vocal tone.

We regret to find that the election of an Assessor to the Council of St. Andrew's University has been conducted on political lines.

M. P. Fischer ("Comptes Rendus") shows that the abysmal fauna of the intertropical Atlantic, though containing many boreal forms, is not exclusively composed of immigrants from the Arctic Seas. The limits in depth of the boreal species increase as we approach the Equator. Thus *Malletia obtusa*, found at 200 fathoms near Norway, occurs at 3200 at the Senegal.

Mr. R. S. Tarr ("Science") gives an account of a tract of "singing sand" which forms a long narrow strip on the coast at Monomoy Point, Massachusetts.

Prof. A. Winchell denies that the period of the inner satellite of Mars furnishes any objection to the nebular hypothesis.

"Science" quotes from the "Transactions of the New Jersey Microscopical Society" (Nov. 19th, 1883) an account of a full-grown dipterous larva (*Sarcophaga carnaria*) taken from the inner ear of a man at Paterson, New Jersey, August 30th, 1883. The author, Dr. S. Lockwood, referred to papers which he had read before the Society in 1880 and 1881, describing larvæ of *S. carnaria* and *Anthomyia canicularis* passed by a man in large numbers. Dr. A. V. N. Baldwin had recently found a cluster of grubs, hard packed, in the external ear of a man in the Bellevue Hospital.

The "New York Sanitary Engineer," as quoted by the "Medical Press and Circular," reproduces the ancient joke of a reporter who mixed up his notes of a meeting on the conversion of the Jews with those of a discussion on the sewage question.

Rev. Dr. McCook has read a paper, before the Philadelphia Academy of Natural Science, on the weaving habits of *Psocus sexpunctatus*. The Psocidæ, a neuropterous group, are the only known insects which spin when in the mature state.

It is said ("Science") that the powder of the Kola nut renders foul water drinkable and harmless. It thus plays in Africa the same part as do the seeds of *Strychnos potatorum* in India.

M. Dieulafait ("Comptes Rendus"), after a prolonged investigation, concludes that the gypsums and ophites are independent in their origin.

The "Zoologischer Garten" records the birth of a mandrill at Hamburg. The facial wrinkles and the brilliant colours of the face were not well marked.

M. J. Chatin ("Comptes Rendus") has been studying a Nematode which is parasitical not in any animal, but in the onion. The species in question belongs to the genus *Tylenchus*.

Says "Light"—"We may depend upon it that when Science and Theology are both sufficiently awake to see a common enemy in Spiritualism, there will be lively times."

A correspondent informs us that a dead owl was recently found near Emsworth with the tail of a rat protruding from its beak. Either the owl had been choked in attempting to swallow the rat (?) or the latter when seized had forced itself into the bird's throat in trying to escape.

Bishop Butler says that communities may go mad as well as individuals. Experience says the same.

The "Geological Magazine" gives a very able notice of "The Connection of the Animal World in Geological Times," by Prof. Gaudry, one of the latest and most important converts from Cuvierism to Evolution.

The late Electrical Exhibition in Vienna has not proved successful from a commercial point of view. The deficiency is said to amount to £6000.

"Science" gives an able and appreciative survey of the life and work of the late illustrious American entomologist, John Lawrence Leconte.

Among the fishes most commonly eaten the cod is poorest in total solid matter, in albuminous compounds, and in fat.

"Les Mondes says of a recent French author, "He is neither a German nor an Englishman, in an age quite infected with Germanism and Britonism. He is himself, with the qualities of the French mind."

M. Delbœuf, a "Dissenting Positivist," complains that "accredited Science does not scruple to constitute ponderable matter of imponderable atoms, polarised matter of non-polarised atoms, variable matter of invariable elements."

The temperature produced by the release of liquefied oxygen is given by M. Wroblewski ("Comptes Rendus") as approximately −186° C.

Certain compounds isolated by M. Pouchet from animal excreta and from putrescent matter, and described in a communication to the Academy of Sciences, occasion in frogs paralysis, with cessation of the reflex movements. The heart is finally arrested in systole.

The Rev. W. H. Dallinger, F.R.S., has been nominated to the chair of the Royal Microscopical Society. The president elect has been distinguished for his researches in conjunction with Dr. J. J. Drysdale, on the life-history of monads, an investigation of the most laborious nature: in some instances the two observers relieved each other at the microscope for a long period, until the observations were completed. One of the most important results of their researches was the discovery of the comparatively high temperatures* which the germs of the most minute Infusoria were capable of surviving, amounting almost to indestructibility, and giving the finishing blow to the doctrine of heterogenesis.

Prof. R. Owen, on retiring from his post as Keeper of the Natural History Department of the British Museum, has received the rank of C.B. Surely a trifling honour, if we consider the exalted merits of the veteran *savant*, and compare him with certain men upon whom it has been formerly conferred!

"Les Mondes" announces the formation of an "International Society of Electricians," the officials and council of which appear, however, to be all Frenchmen.

It is said that Prof. Huxley, P.R.S., will receive the offer of a baronetcy.

The epidemic of trichiniasis at Emersleben, and some neighbouring villages, extended to upwards of 370 persons, of whom 51 died. All the sufferers had eaten "hack fleisch," *i.e.*, pork chopped up fine, and consumed perfectly raw!

M. A. Cornu ("Comptes Rendus") records a *white* rainbow, seen at Courtenay, on the morning of November 28th.

M. Ch. Trepied, in a communication to the Academy of Sciences, states that the spectrum of the comet Pons-Brooks agrees in its visible portion with that of the flame of alcohol.

Mr. "Stuart Cumberland" has been performing in Edinburgh under the combined auspices of Law, Physic, and Divinity.

* *Tetramitus rostrata* could resist a temperature of 250° F. (121° C.); *Dallingeria Drysdali*, 220°; *Cercomonas typica*, 260°; and *Heteromita rostrata* and *H. uncinata* passed unscathed through a temperature of no less than 300° F. (148° C.), which represents the highest limit that has yet been obtained.

According to Dr. Hahn ("Comptes Rendus") the Yakanas, a tribe inhabiting the neighbourhood of Murray Straits and of Beagle Channel, celebrate a yearly festival commemorative of the revolt of the men against the women, who had previously held the chief authority in the family, and who possessed the secrets of sorcery.

The presence of the *Phylloxera* in the vineyards of Victoria is unfortuately a demonstrated fact. A Sub-committee of the Entomological Society have made a minute examination of roots of vines sent over for this purpose by the Victorian Minister of Agriculture, and have found upon them numerous specimens of the insect in its "under-ground" stage.

The interesting collection of Lepidoptera formed by the late Prof. Zeller, of Stettin, has become the property of Lord Walsingham.

We learn from a contemporary that a graduate in medicine being asked "What is Semeiology?" answered "A description of the Spermatozoa."

According to the Astronomer Royal ("Monthly Notices of the Royal Astronomical Society") the green band of the spectrum of the Pons-Brookes comet does not coincide with the green band of the alcohol spectrum, but lies far to the blue of it.

The Anthropometric Committee of the British Association report that Fellows of the Royal Society are more than 2 inches above the average stature. (We wish a separate measurement could have been made of that select body of the Fellows who communicate papers to the "Transactions" and the "Proceedings."

M. Marcel Deprez contends that electricity is not one of the forms of energy, but is merely its vehicle.

Dr. W. Jones ("Science") records that a spider remained shut up in a paper box for 204 days without any kind of food. She did not appear feeble or emaciated until within three days of her death.

Mr. W. H. Hudleston, F.G.S. ("Mineralogical Magazine") thinks it possible that the diamonds in South Africa are the result of reactions which have taken place between the hydrocarbons of the shales and the hydrous magnesian silicate, under peculiar conditions of temperature and water-gas pressure.

In the advertisement list of an eminent American firm of educational publishers we find 30 books on Latin, 19 on Greek, 11 on Music, and 3 on Science,—to wit, a treatise on vibratory motion and sound, an elementary manual of Physics, and a set of questions on Physics. Do these figures show the relative position which the heads of colleges and schools allot to Science?

"Science" remarks that "Science must be almost as much popularised to be made accessible to all scientific readers as to be readable by the educated public who were never in a laboratory."

The skeleton of an unusually large mastodon ("Kansas Review of Science") is said to have been found near Woodstock, in Canada. The lower jaw is 30 inches long, and must have weighed upwards of 150 lbs.

Dr. A. Gœtte considers the cause of death in the simplest polyplastides to be "propagation by germs capable of development."

A. Burgerstein ("Deutsche Botan. Gesellschaft") has observed that the petals of the Compositæ are capable of absorbing water, the lower surface more readily than the upper.

"Science" gives an interesting account of the marine laboratory in connection with the Johns Hopkins University.

The "Popular Science Monthly," in an otherwise able article against the predominance of classical studies in education, says, "A despotic paternal Government has Church-and-State reasons for maintaing a dead-language culture as a national policy."

Mr. A. G. Boardman, in the same paper, gives facts supporting the view that Dipterous insects are carriers of disease.

Herr Westermaier ("Berichte Deutsch. Botan. Gesellschaft") traces the ascent of water in plants mainly to endosmotic action.

Professor Owen, in a recent work, maintains that the dorsal aspect of a caterpillar or a crab really corresponds with the ventral aspect of a vertebrate animal. The pineal and pituitary bodies are the residue of the deutostome or invertebrate mouth.

Professor Helmholtz calculates that the solar system has been in existence for 500 million years,—a more liberal allowance than that of certain English physicists.

Professor Heer maintained that the true vine is an archiæval inhabitant of Europe.

The first atmospheric wave caused by the Sunda eruption reached Berlin ten hours after the event; sixteen hours later came a second wave, *viâ* America; and thirty-seven hours afterwards a third, by the direct way. Minute and unusual fluctuations of the barometer were still traced until September 4th.

The "Banner of Israel" announces the discovery of certain remains of Noah's ark.